SCIENCE.

FRIDAY, AUGUST 17, 1883.

THE AMERICAN ASSOCIATION AT MINNEAPOLIS.

THE number of people who take an interest in scientific discovery is very great. We may assume that it far exceeds estimates based on the support given to scientific periodicals and societies. The question is not of thousands, but of hundreds of thousands. Of a report of Professor Tyndall's lectures on light in New York, there were sold over a half-million copies. That was ten years ago: the popular interest in science has vastly increased in the interval. This is shown by the gain of membership in the American association for the advancement of science, being within the last four years as great as in the previous thirty-one years.

Compared with what may be called the scientific following, the number of workers in science is small. Upon that following the workers must depend for recruits, and, directly or otherwise, for support. Science must lean on her friends: they are numerous, but few of them give help. There are large and rich communities where the local developments are on a par with the Pickwick club. The men and means for good work are not wanting, but the impulse is. 'Oh for the touch of a vanished hand,' like that of Louis Agassiz, to warm the dormant interest into life!

For this purpose the American association is an effective agency. It unites in one body the workers and those who are not professionally engaged in scientific pursuits. Its management should be and is favorable to the desires of both classes. In the social features of its meetings, all share alike, and perhaps with equal zest. But the workers regard the meetings chiefly as the occasions for hearing and reading 'papers.' Teachers, who form a large part of the membership, seek the most recent things of knowledge to add to their capacities for instruction. A majority of the attendants at the meetings come simply with a wholesome curiosity for the novelties of science.

The production and delivery of 'papers' at these meetings give rise to some queries. Is there any natural reason for expecting genius to burst into blossom in August rather than in any other month? If a man of science is diligently pursuing some line of research, may not the light that never was on sea or land break upon him in any other of the fifty-two weeks than the one when he can present it to the annual meeting? If he keeps back his announcement of progress or discovery, or if he brings it forward before he is fully prepared, does he not harm the cause of science and himself?

The 'papers' are of necessity often technical and uninteresting to all except experts in some special line. At one of the meetings a certain mathematician stated the case bluntly, thus: "I shall read my paper by title only, as there is nobody but myself here who can understand it." The rapidity with which a crowd of members thins out when the reading of a technical paper fairly begins, is at least suggestive. Nor should the departing crowd be denounced as simply unworthy of the pearls spread before them. They will stay if the paper has only a fair trace of popular interest. Doubtless many of those who leave the association in their first year of membership are disappointed. They had hoped for something not quite so 'dry.' Yet, if the reading of papers were dropped, the association would fail to gather the workers of science at its meetings.

Plans have at times been considered for securing addresses from men who are known as popular speakers, capable of attracting large audiences, especially if aided by suitable apparatus for the display of experiment. In various ways such a course might add largely to the resources and influence of the associa-

tion. What is vastly more important, it would rouse an enthusiasm for science at the locality of the meeting, which, if rightly fostered, would give permanent results.

The association has sought to meet some of these wants and difficulties by creating a larger number of sections, each of which has a presiding officer, who is expected to deliver a formal address. This is an advance, but only a half-way measure. The papers increase in number every year; and the several sections must all work at once and arduously to finish their reading in the allotted time. To many a member, even to a specialist who may be engaged in two distinct lines of research, comes the disappointment of missing the hearing of valuable papers when two or three are delivered simultaneously.

Many of these features must appear prominently at the present meeting. The attendance will consist in greater proportion than usual of the popular element. The membership is now so large that there is no risk of the meeting being insignificant in size, as at Dubuque in 1872. But, since Minneapolis is the farthest point to the west yet tried, its distance must withhold many familiar faces. After this, we shall know better whether the kind invitations of San Francisco may be accepted two or three years hence. Next year the meeting should not be too far from the British association at Montreal.

At least eight addresses will be given by presidents of sections, — excellent in their kind, but not quite a substitute for thoughts that breathe and words that burn. If free and wide discussion could be encouraged at these meetings, the retiring president's address would now give abundant occasion. Dr. Dawson hits hard where he thinks he sees a crevice in the armor of the evolutionists or of the glacialists, and many will chafe if there is no immediate opportunity to return his thrusts. But, while it may fail of excitement, the meeting at Minneapolis is very enjoyable. The city and vicinity are picturesque and delightful. The hospitality of the west is as broad as its prairies W. C. W.

THE IGLOO OF THE INNUIT.—I.

The Esquimaux of the arctic regions of North America call themselves 'Innuits,' and their winter-houses, built of ice and snow, 'igloos.' This short explanation may be needed to make clear my somewhat obscure title.

These strange huts have been incidentally described by many travellers in the accounts of their arctic explorations. But beyond the fact that they are rude domes of snow, in which these polar people live for the greater part of the year, little is known of the manner of their construction, their internal arrangement, or of the conditions which have led to their existence.

The many inquiries I have been called upon to answer in regard to these northern cabins, and the misconceptions I have found even among the better informed of my questioners, have led me to believe that an account of the igloo as I saw it during my life with the Innuits would be of interest.

The origin of the igloo can only be guessed from the few facts we know of early man. I will not discuss the ethnological problem which would identify the Innuit of the present day with the cave-men of Europe, but, assuming that it is true, will sketch a possible history of the ice-hut.

These cave-men are known to have existed along the edges of the *mer de glace*, which, during the ice period, overspread Europe, and buried it as Greenland is probably buried at the present day. What caused this great flow of frigidity to the south, or its retrogression to the north, it is needless to consider; suffice it to suppose that our hyperboreans followed it in all its migrations. The earliest evidences of their history are those they left in the caves of middle Europe when the glacier extended nearly to the Alps and Pyrenees, beyond which, with its outlying polar fauna of cave-men, cave-bears, cave-hyenas, mammoths, and reindeer, it never extended.

These caves were the work of nature. When these people lived in their vicinity, it is probable that they knew no other habitations, winter or summer, and disputed their possession with the many animals whose bones are found beside the implements and bones of the cave-men themselves.

As the *mer de glace*, with snail-like pace, withdrew northward, it was followed by these children of the cold (the cave-men), driven, as some suppose, by the more powerful river-drift men, or following that climate which was the more congenial.

The cave-men in their retreat, tightly held by other tribes or climatic temper, when they reached the older geologic formations which no longer gave them the welcome shelter of nature's rude houses (the dreary caves), must have looked for it from other means; and these were only stones and snow-banks. The former may have been used for their more permanent homes; but the cold interiors of stone huts in such a climate must soon have driven them to the more comfortable and easily built houses that can be excavated from a snow-bank, and so greatly resemble their old cave-homes.

During the first part of their retreat, the cave-men, cave-men no longer, were in a hilly, half-mountainous country, — a character of surface favorable to the formation of snow-drifts large enough to allow of pit or excavation, in which a family could comfortably reside. Here, then, was the first igloo, rudely cut into some protecting bank of snow, its walls knowing no other construction than that of nature. Such rough types of arctic architecture are still to be found among the mountains, where wood is unknown.

As the migrating sea of ice debouched upon the shores of the Arctic Sea, and withdrew its icy blanket from these more northern regions, the ancient arctic man found himself, as he reached those limits near the White Sea and the mouth of the Petchora, in a flatter country. The snow-drifts no longer lay in such colossal depths. They were direct functions of the surface, and flattened with it. It was no longer possible to construct a deep enough house by simple excavation. The problem was probably met by digging as far as possible, and completing the structure with banks, which in time were made of blocks of snow; for the snow of the arctic winter is not of that plastic nature which will allow one to fashion it at will, as schoolboys their forts and imitation-men, but dense and compact from the extreme cold and the packing wind. Such were the first typical and perfect igloos, a direct outgrowth of the level barren lands of the arctic zone, — features which yet determine its geographical limits.

Arctic man stopped on the shores of the sea, for in the rude means at hand he could follow the ice no farther. There was another migration to the north, which was to affect the character of his dwelling: this was the migration of the forests. As soon as wood reached his door, either by direct migration of the forests or by drifting down the great northward-trending rivers, he would naturally use it in the construction of his permanent houses, as we see to-day among the natives thus situated. The igloo was probably driven from Europe, then from Asia, and is now confined to certain localities of North America.

From writing of the igloos of the Innuits, the natural inference is, that the geographical boundaries of the two would be the same. The Innuits reach from Bering Straits (and even southward along the Alaskan coast and outlying islands) nearly to those of Belle Isle, following the sinuous coast of North America at irregular intervals. They populate the western shores of Greenland, and once occupied its eastern side. Yet this vast stretch of ocean-line must be shorn of the greater portion of its length before we can narrow it down to the part occupied by the igloo-building Innuits.

The data I have already given restricting the igloo to the barren grounds devoid of even driftwood, and the fact that nearly all Esquimaux tribes are a seacoast-abiding people, will assist us in a rough but fair approximation to its limits, — limits which can be readily made clear by reference to a map of the arctic regions of North America. The mouth of Mackenzie is about the dividing-line of the timber to the west and the barren country to the east. For considerable distances on both sides of its mouth, there is a good supply of driftwood. Where this driftwood ceases on the east is the western limit of the igloo, probably fifty to one hundred miles from the river. From this point they are found all along the coast, on the portions of the Parry Islands occupied by Esquimaux, the shores of Hudson's Bay and Straits as far as Marble Island, of Cumberland Gulf, and many of the estuaries of Baffin's Bay. The limit on the south is, I believe, Hudson's Strait, and on the east Baffin's Bay.

The time during which igloos may be built depends on the length of the winter. In summer the natives use a tent of seal or walrus skin.

The pole of greatest cold is placed by Bent to the north of the Parry Islands, nearly upon the eightieth parallel, and in about 100° W. longitude. I believe the thermometric observations made in the arctic regions, straggling as they have been, go far towards showing that the magnetic and thermal poles are the same. This would bring the lowest temperatures six hundred miles to the south of the position assigned by Bent. Wherever it may be, there would the igloo have the longest existence for the year.

In the winter of 1878, being near Depot Island in North Hudson's Bay, we moved into igloos on the 1st of November. On King William's Land, next spring, we abandoned snow-houses, and took to tents on the 17th of June, having lived an igloo-life for seven months and seventeen days. That winter upon King William's Land we reared our first igloo on the 25th of September, being one month and five days earlier than at Depot Island the previous season. This would give a total of igloo-life for the southern part of King William's Land of eight months and twenty-two days, or nearly three-fourths of the year. This is the nearest to the pole of greatest cold (be it the magnetic pole or according to Bent) that any white men have lived *à la Innuit*. Assuming these two physical poles to be identical, and our position having been so near them, — being really only about a hundred miles distant, — we must have experienced about the maximum of annual igloo-life. Returning to North Hudson's Bay in the spring of 1880, we, as well as the majority of the Esquimaux living around Depot Island, moved into tents about the middle of May, giving igloo-life for North Hudson's Bay something over half the year, which is probably near the minimum.

While, of course, climatic causes principally determine the annual longevity of the snow-house, they are not the only ones. As soon as the spring thaws commence tumbling in the igloos, or making their structure insecure, the native would gladly avail himself of a tent; but this he cannot do, unless there be a clear spot somewhere near, on which it can be pitched. It may be a number of days from the time he would accept tent-life before the hilltops or ridges commence peeping through their winter covering. The inland ridges, higher and more marked, covered with black moss, which, once through the crust, makes sad havoc with the snow, appear much sooner than those facing the sea, which are flatter, enabling the inland reindeer hunters to occupy their tents earlier than the seal or walrus hunters of the coast. Some igloo-builders will wait until they can kill enough seal to make a new tent before using one. The Ooqueesik Salik Esquimaux of the Dangerous Rapids of the Great Fish River can be said to be practically without tents, securing nothing, or almost nothing, from which to make them. They hold to the shelter of an igloo late in the spring, and seek it as soon as one can be made in the early winter.

(*To be continued.*)

ON THE DEVELOPMENT OF THE PITUITARY BODY IN PETROMYZON, AND THE SIGNIFICANCE OF THAT ORGAN IN OTHER TYPES.

In the *Quarterly journal of microscopical science* (xxi. 750) I published a brief preliminary account of the development of the pituitary body in the lamprey, stating that it was formed from a part of the nasal sac. This account of a method of formation so entirely different from any thing that was known among the vertebrates was received with incredulity by Balfour, who says (Comp. embryology, ii. 358), "I have not myself completely followed its development in Petromyzon, but I have observed a slight diverticulum of the stomodaeum which I believe gives origin to it. Fuller details are in any case required before we can admit so great a divergence from the normal development as is indicated by Scott's statements." These fuller details have long been nearly ready for publication, but I have been prevented by circumstances from issuing them. I hope shortly to continue my series of studies on the embryology of Petromyzon, but, in the mean time, think it advisable to present this preliminary account.

My friend, Dr. Dohrn of Naples, has lately investigated this subject, and has come to the conclusion that neither Balfour nor myself can be correct, but that the pituitary body arises from an independent invagination of the epiblast between the nasal epithelium and the mouth (*Mitth. zool. stat. Neapel*, iv. 1 heft). On examining Dohrn's figures, however, I was much pleased to find that his disagreement with me is rather about terms than facts; for these drawings correspond almost exactly with those that I have already published, and many more as yet unpublished.

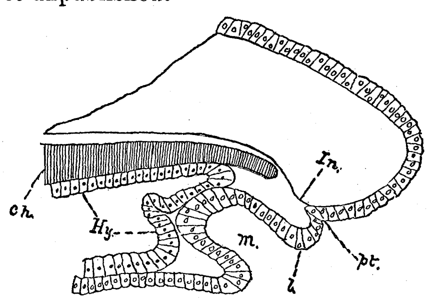


FIG. 1. — Sagittal section through head of lamprey embryo. *m*, mouth; *pt*, pituitary invagination; *In*, infundibulum; *Hy*, hypoblast of throat; *ch*, notochord; *l*, upper lip.

The development of the pituitary body, as far as I have been able to trace it, is as follows. Shortly before hatching, the mouth is formed by a deep invagination of the epiblast (see fig. 1,

taken from my article in the *Morphol. jahrb.*, vii). The upper lip is somewhat rounded in longitudinal section, and bounded anteriorly by a very slight depression, which is the beginning of the pituitary body; but, as this is also the beginning of the invagination to form the nasal sac,[1] I have preferred not to separate them, as Dohrn has done. In the next stage (fig. 2)

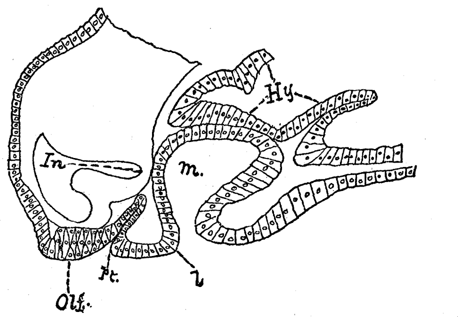


FIG. 2.—Section through head of an older embryo just before hatching. *Olf*, olfactory epithelium. Other letters as in fig. 1.

the nasal epithelium has become much thickened, the pituitary involution deeper, and the upper lip elongated so as to become triangular in section. At this time the cranial flexure has reached its maximum; though it is far less than in most other groups, owing to the relatively small size of the fore and mid brains. The mouth is ventral in position, corresponding very closely to the selachian mouth in position and shape.

Shortly after this, the upper lip begins that remarkable series of transformations to which, as I long ago pointed out, many of the most striking peculiarities of the cyclostome organization are due. The posterior edge of the lip elongates rapidly, becoming triangular in section; while the whole anterior part of the head rotates forwards, thus tending to correct the cranial flexure, and bringing the mouth to point somewhat forward as well as downward. By this process the edge of the lip, which in fig. 2 is directed backwards, now comes to point downwards (fig. 3); at the same time, the opening of the nasal pit points forwards instead of downwards. The involution for the nasal passage and pituitary body has now become a long tube of cells, which transverse sections show us to be perforated by a small lumen. The end of this cellular tube reaches to the infundibulum, with which it lies in close contact. This portion will give rise to the pituitary body. Up to this time there has been no line of separation between the pituitary involution and the nasal epithelium; but when the process of rotation of the upper lip, and correction of the cranial flexure, is completed, the edge of the lip points directly forward, having passed through an angle of 180°, and the opening of the nasal sac is on the dorsal instead of the ventral surface of the head. At this time a fold appears below the olfactory epithelium, separating it distinctly from the pituitary passage.

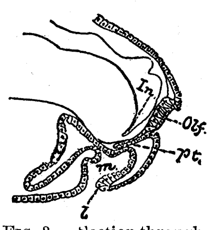


FIG. 3.—Section through head of a very young larva of the lamprey. Letters as before.

The pituitary body is formed from part of the epithelium of this passage, and consists of solid follicles, separated by connective tissue. According to Dohrn (loc. cit., p. 178), this body is not constricted off from the passage or nasal sac at any time during larval life. I have not been able to satisfy myself, as yet, upon this point; but I am not inclined to agree with this view.

As to the morphological significance of the pituitary body, many views have been propounded, some of them bearing upon the question of the origin of the vertebrates. Some writers have contended that the conario-hypophysial tract through the brain is the remnant of the old mouth and gullet, which, in the ancestors of the vertebrates, passed through a ring of nervous tissue, as in the annelids. Space will not permit a discussion of this hypothesis; nor is such discussion necessary, as Balfour (Elasmobranch fishes, p. 170) has stated the insuperable objections to the view. Dohrn, in the pamphlet already quoted, adopts a view somewhat like one originally propounded by Götte, and adds a suggestion of his own. He considers the entire blind nasal sac of the lamprey to belong to the pituitary body, and that this sac has arisen from the coalescence of a pair of gill-slits. This hypothesis is but the carrying-out of the theory so ably advocated in the very suggestive pamphlet 'Ueber den ursprung der wirbelthiere.' But, until it can be shown that the vertebrate mouth is a new formation, the existence of pre-oral gill-clefts hardly merits discussion. I reserve for a later paper the consideration of the origin of the vertebrate mouth, — a question which is the turning-point of the solution of all these problems.

Balfour has suggested an explanation of the pituitary body. "It is," he says (p. 359), "clearly a rudimentary organ in existing craniate vertebrates; and its development indicates, that when functional it was probably a sense-

[1] By nasal sac, I mean the blind passage, as distinguished olfactory epithelium.

organ opening into the mouth, or else a glandular organ opening into the mouth." It seems to me that the facts of its development in Petromyzon negative this hypothesis. It is there seen to have no connection with the mouth; nor is this mode of development so entirely exceptional as it would at first seem. Of all known embryos of craniate vertebrates, the lamprey has perhaps the smallest brain and the least cranial flexure; which state of things allows space for a distinct invagination from without to reach the infundibulum. In the Amphibia this is seen to a less degree: the invagination for the pituitary body is formed before the appearance of the

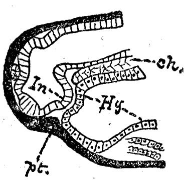


FIG. 4. — Section thro' head of embryo of Bombinator (after Götte). Letters as before.

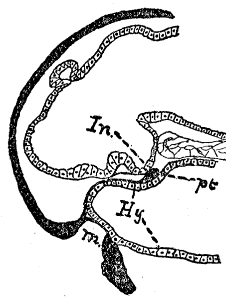


FIG. 5. — Section through head of young tadpole of Bombinator (after Götte). Letters as before.

mouth, and just above it; so that, when the mouth appears, the two have an apparent connection, being crowded together by the increased cranial flexure. In other types — such as the selachian, bird, mammal, etc. — the brain acquires a very great size in early embryonic stages, and the cranial flexure is consequently very much increased. In these cases almost the only possible way for an epiblastic invagination to reach the infundibulum is from the epiblast of the mouth. If the reader will compare the figures given above for the lamprey with those from Götte (figs. 4 and 5) for the amphibian and that from Balfour for the selachian (fig. 6), these progressive changes will at once be clear. If embryological evidence counts for any thing, it would therefore seem extremely probable that the connection of the pituitary body with the mouth is only a secondary one, brought about by the greatly increased cranial flexure in the higher types.

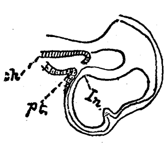


FIG. 6. — Section thro' head of embryo of Pristiurus (after Balfour). Letters as before.

Assuming that the invagination originally took place independently of the mouth, such a secondary connection would be almost a mechanical necessity of the great brain-growth.

Now, while I am not prepared to follow Dohrn in maintaining that the entire blind nasal sac below the olfactory capsule of Petromyzon really belongs to the pituitary body, yet I quite agree with him that the connection of the pituitary body with the olfactory organ is a secondary one. I have, in a former paper, stated the reasons for believing that the unpaired condition of the olfactory organ in the Cyclostomata is not primitive, but secondary, caused by the coalescence of two originally distinct pits. Now, if there were an independent invagination in the median line of the head, the causes which brought about the union of the two nasal sacs would also cause the latter to coincide with the pituitary involution. This is just what I conceive to have happened.

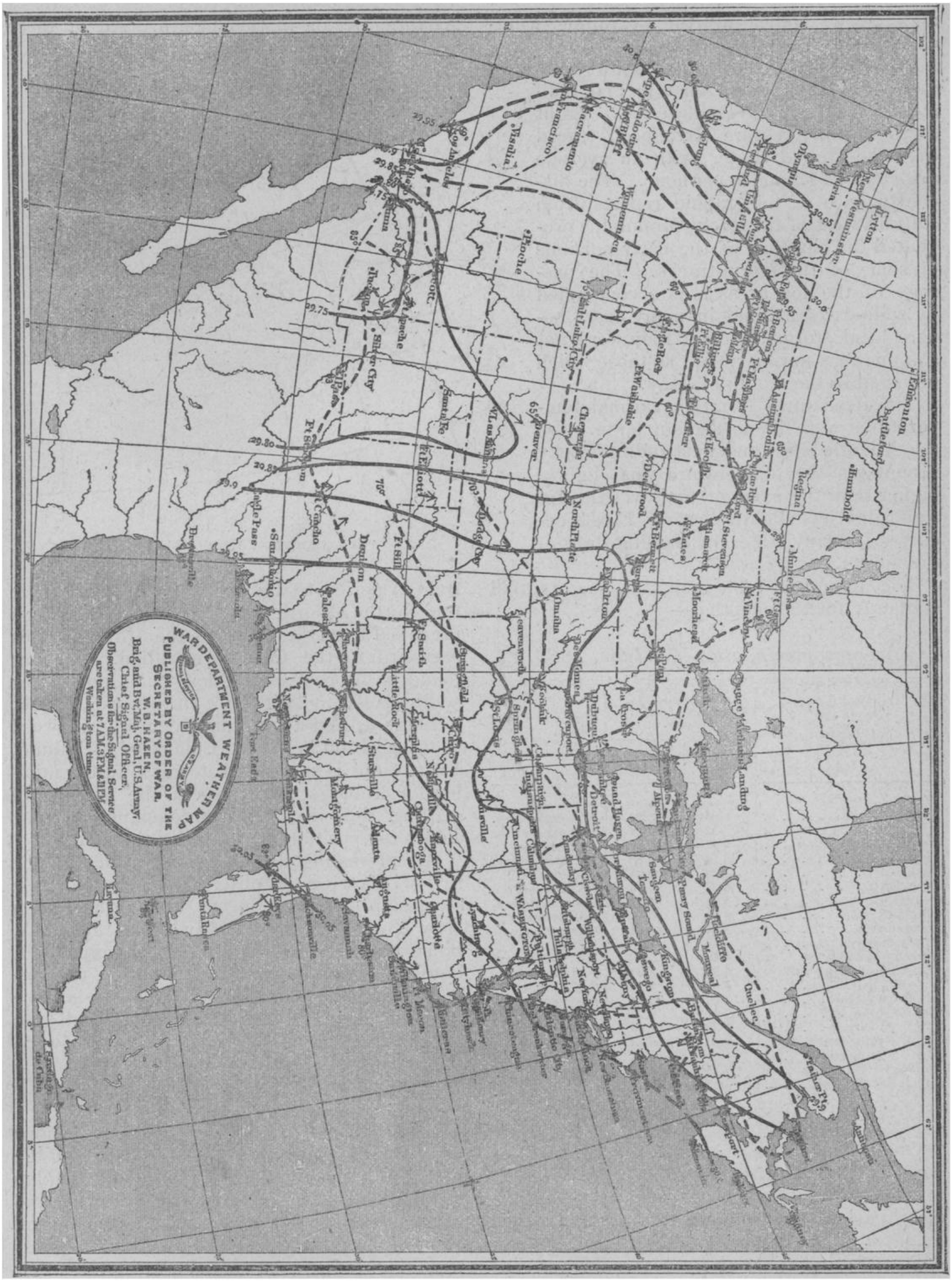
If the above reasoning be correct, the fact would seem clear, that the pituitary body is the remnant of some originally independent organ, which opened, not into the mouth, but on the surface of the head. Almost certainly this organ belonged to the invertebrate ancestor of the vertebrates. What its function was, is a difficult problem. Dohrn's hypothesis that it was formed by the coalescence of a pair of gill-clifts is untenable, not only for the reasons already given, but on account of the invariable epiblastic origin of this organ, while gill-clefts always arise in the vertebrates as outgrowths of the hypoblast. Perhaps we may modify Balfour's suggestion, and assume tentatively that it was a sense organ or gland which, having lost its function, has become rudimentary. At all events, it will be a step gained if we can establish the fact that the pituitary body is an organ originally independent both of the mouth and of the olfactory apparatus. W. B. SCOTT.

Morphological laboratory, Princeton, N.J.,
July 5, 1883.

THE WEATHER IN JUNE, 1883.

THE monthly weather review of the U. S. signal-service contains in usual detail reports from all portions of the country of the weather conditions which characterized the month of June. There were no unusual meteorological features; the month exhibiting the 'average weather,' as far as this term can be realized. The destructive floods in the lower Missouri River, and in the Mississippi River between St. Louis and Cairo, the unusual rainfall in that section, and severe local storms in many of the states, are the special events of note.

The mean distribution of barometric pressure is illustrated by the accompanying chart, which also contains the mean isothermal lines,



MONTHLY MEAN ISOBARS, ISOTHERMS, AND WIND-DIRECTIONS, JUNE, 1883. REPRINTED IN REDUCED FORM BY PERMISSION OF CHIEF SIGNAL-OFFICER.

and arrows indicating the prevailing wind-directions. The pressure conditions are quite normal, the regions of highest mean pressure being the South Atlantic and Gulf states, and the North Pacific coast. Eight areas of low pressure have been traced over the United States, with an average velocity of 24.2 miles per hour. The discontinuance of telegraphic reports from stations west of the Rocky Mountains prevented the charting of the early portions of some of the storm-tracks. The passage of the low areas was accompanied by wide-extended and in many cases severe local storms, though they were not so numerous nor so violent as those which occurred in the month of May.

The departures from the normal temperatures were in no section large. On the Atlantic coast and west of the Rocky Mountains the temperature was slightly higher than the average, and over the interior districts slightly lower. Frosts occurred in many states in the first days of the month.

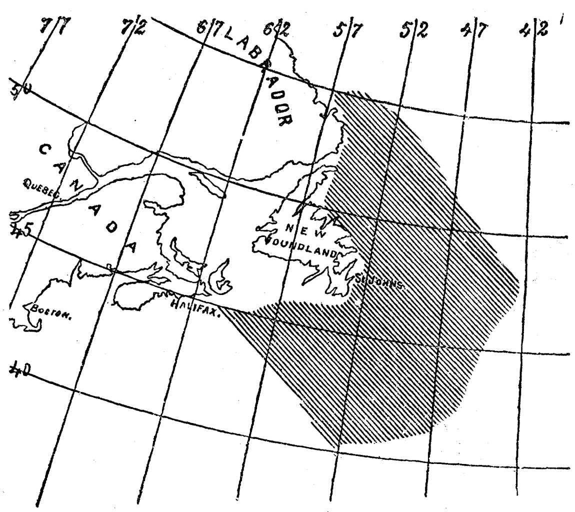
The following table contains the rainfall statistics for the month: —

Average precipitation for June, 1883.

Districts.	Average for June. Signal-service observations.		Comparison of June, 1883, with the average for several years.
	For several years.	For 1883.	
	Inches.	Inches.	Inches.
New England	3.60	3.36	0.24 deficiency.
Middle Atlantic states	3.52	5.22	1.70 excess.
South Atlantic states	4.57	6.49	1.92 excess.
Florida peninsula	5.70	4.80	0.90 deficiency.
East Gulf	4.29	4.91	0.62 excess.
West Gulf	3.37	3.73	0.36 excess.
Tennessee	4.34	3.49	0.85 deficiency.
Ohio valley	4.64	4.21	0.43 deficiency.
Lower lakes	3.26	4.04	0.78 excess.
Upper lakes	4.47	5.38	0.91 excess.
Extreme north-west	4.10	2.50	1.60 deficiency.
Upper Mississippi valley,	5.82	5.98	0.16 excess.
Missouri valley	5.06	7.98	2.92 excess.
Northern slope	2 53	3.43	0.90 excess.
Middle slope	2 01	2.27	0.26 excess.
Southern slope	3.26	1.70	1.56 deficiency.
Southern plateau	0.40	0.03	0.37 deficiency.
North Pacific coast	1.50	0.04	1.46 deficiency.
Middle Pacific coast	0.18	0.00	0 18 deficiency.
South Pacific coast	0.02	0.04	0.02 excess.

On account of the excess of rain in the Missouri valley, disastrous floods occurred in the latter part of the month. At St. Louis the river reached the highest point since the establishment of the signal-service station. Much delay was experienced by the railways centring in St. Louis and Kansas City.

Three depressions only are charted upon the Atlantic Ocean in this month. All of these are in the eastern portion, and none are traced



ICE-CHART FOR JUNE, 1883.

from America to Europe. The weather over the North Atlantic was fair; but dense fogs prevailed from the coast of the United States eastward to the fortieth meridian. Ice was found as far east as 42° longitude, and as far south as 40°.5 latitude. During the month, icebergs drifted about three degrees eastward of the position in May. Compared with June, 1882, there is a marked decrease in the number of icebergs, and also in the amount of drifting field-ice. The accompanying chart shows the position of the ice in the month.

An interesting diagram is published in the review, showing the observations made on the steamship Assyria during her voyage from New York to Bristol, May 27 to June 11. Some of the symbols used are unexplained, however. The marked features are the rise in temperature immediately after leaving the Atlantic coast and the corresponding fall east of the fiftieth meridian, the agreement between the temperature and pressure curves, and the agreement between the temperatures of the air and sea-water.

Minor displays of auroras at various stations were reported during the month, and on the 30th an extensive but not brilliant display was noted. The number of sun-spots and groups was large. The record of halos, mirage, and meteors is large; and two water-spouts were reported, — one on Lake Erie, the other on Lake Monroe, Fla.

The verification of the tri-daily indications

shows the average of 85.1 %. Of the cautionary signals displayed, 80.4 % were justified by winds exceeding twenty-five miles an hour at or within one hundred miles of the station.

THE FALL OF A BALLOON.[1]

IN the August (1882) number of *l'Aeronaute*, accounts were given of the different ascents made on the 14th of July of that year. Among these ascents that of Cottin and Perron was of especial

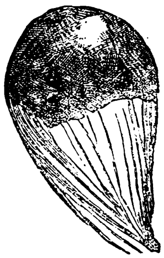


FIG. 1.

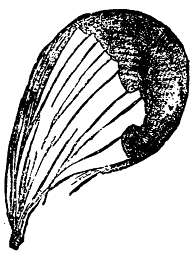


FIG. 2.

interest, not because of the length of the voyage, but from its brevity, and on account of the fall which ended it. The balloon had barely started from Paris when a rent was formed in the upper part, and the balloon descended at Saint-Ouen. This occurrence is not entirely unknown; but that which does not

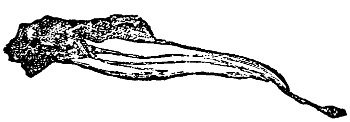


FIG. 3.

happen often is, that an artist, Mr. Jacque, chanced to be at his window, and was able to make rapid drawings of the balloon during its descent, and Mr. L. Gillon viewed the accident from the Place Wagram, and made three drawings.

Mr. Cottin, thinking that the aeronauts had not attached sufficient importance to his ascent, has published an account of it in a brochure, illustrating it with the drawings of Jacque and Gillon. He begins his statement, "It was sixteen minutes past four. The wind was blowing violently from the south-east. The temperature was 28° C. At starting, the voyagers felt nervous, and noticed some excitement in the movements of those who were assisting. Nevertheless, they started, saluting the crowd, who responded as only a sympathetic Parisian crowd knows how. They rose over the building which forms the corner of the Place Wagram. Thirty kilograms of ballast was thrown out; and, relieved of this weight, the balloon shot up. With one bound it was four hundred metres; another, and it had reached a height of six hundred metres. At this time it was just twenty-four minutes past four. The aeronauts felt that the balloon seemed to stop. They were told afterwards that they began to turn. Cottin felt a trembling of the basket. Some seconds passed. Then the noise of the flapping silk was heard."

FIG. 4.

The balloon was torn when at a height of seven hundred and three metres, as shown by a pocket barometer which Cottin had with him, and saved in good condition. For the first hundred and twenty metres of the fall the motion was regular. Then a swinging motion began, and finally the fall

FIG. 5.

increased in speed. The oscillations increased enormously, and the basket swung through the air with a dizzying velocity. At times the balloon took up an almost horizontal position in the direction of the wind. This swinging continued till a point within a hundred and twenty or a hundred and thirty metres of the earth was reached. From this point the fall was nearly vertical, as the silk had formed itself into a parachute. During this period Mr. Perron threw out the last of the ballast, the guide-rope, and cut the cords of the anchor. Led by Perron's example, Cottin threw over a bottle of cold coffee, which, he remarks, 'might have injured or even disfigured them.'

[1] Taken, with the illustrations, from *l'Aeronaute*, June, 1883.

Suddenly, without any shock, the basket seemed to drop from under their feet. A moment later they were violently thrown down by the sudden stopping of their fall. It was twenty-seven minutes past four. The ascension had lasted eleven minutes, and two minutes were occupied by the fall of seven hundred and three metres.

They found themselves suspended about two metres from the pavement in the courtyard of a house in Saint-Ouen, the ropes and material of the balloon having caught on the roof. The yard was not more than four metres long by three wide. To complete their good luck, there was a flight of steps which gave them an easy means of reaching the ground.

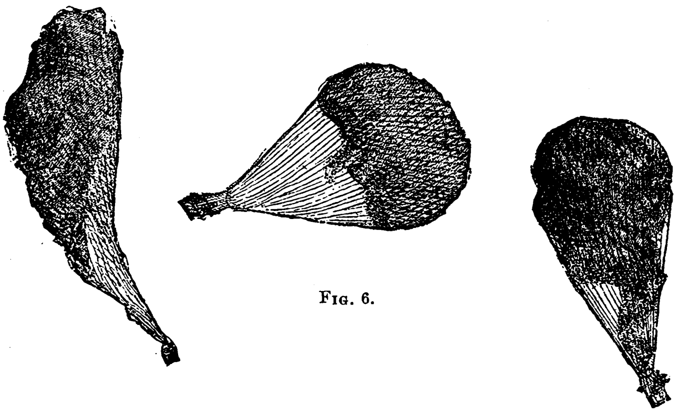


FIG. 6.

Mr. Jacque was in his studio, and saw the balloon in the air. Seeing that something unusual was happening, he seized a pencil, and hastily drew the successive forms which are reproduced in figs. 1 to 4. As to the drawings, he says, "I could only indicate very imperfectly the ropes and basket, which I could hardly see. It is necessary to remark, that the phases represented ought to be supposed as following closely one another, and constantly changing. I suppose that the time during which the fall was visible to me was about one minute, and the distance fallen five hundred metres. At the moment when I saw the balloon taking the last form (fig. 4), it was descending more rapidly, and disappeared behind the left slope of Montmàrtre. It did not seem more than one kilometre distant from me; but in this I was mistaken."

The sketches (fig. 6) of the fall as seen by M. L. Gillon are not accompanied by any explanation.

The figures are of interest as showing the form which a balloon takes when forming itself into a parachute, and give some indication of the resistance offered by the air. The parachute was doubtless of an imperfect form, and offered too great a resistance. It had, moreover, the fault of not having a central opening, on which account the air could only escape laterally, and gave rise to the fearful oscillations. In an actual parachute the central hole, of large size, allows easy escape to the air, and the oscillations are slight. It can almost be said that the resistance of a parachute increases with the size of the opening.

The balloon tore on its upper side on account of the disproportion in the ropes. The lower part, reversing, formed a closed parachute. It is not singular that the balloon should have taken such strange shapes while falling.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

ADDRESS OF THE RETIRING PRESIDENT, DR. J. W. DAWSON, AT MINNEAPOLIS, AUG. 15, 1883.

SOME UNSOLVED PROBLEMS IN GEOLOGY.

MY predecessor in office remarked, in the opening of his address, that two courses are open to the retiring president of this association in preparing the annual presidential discourse, — he may either take up some topic relating to his own specialty, or he may deal with various or general matters relating to science and its progress. A geologist, however, is not necessarily tied up to one or the other alternative. His subject covers the whole history of the earth in time. At the beginning it allies itself with astronomy and physics and celestial chemistry. At the end it runs into human history, and is mixed up with archeology and anthropology. Throughout its whole course it has to deal with questions of meteorology, geography, and biology. In short, there is no department of physical or biological science with which geology is not allied, or at least on which the geologist may not presume to trespass. When, therefore, I announce as my subject on the present occasion some of the unsolved problems of this universal science, you need not be surprised if I should be somewhat discursive.

Perhaps I shall begin at the utmost limits of my subject by remarking that in matters of natural and physical science we are met at the outset with the scarcely solved question as to our own place in the nature which we study, and the bearing of this on the difficulties we encounter. The organism of man is decidedly a part of nature. We place ourselves, in this aspect, in the sub-kingdom vertebrata, and class mammalia, and recognize the fact that man is the terminal link in a chain of being, extending throughout geological time. But the organism is not all of man; and, when we regard man as a scientific animal, we raise a new question. If the human mind is a part of nature, then it is subject to natural law; and nature in-

cludes mind as well as matter. On the other hand, without being absolute idealists, we may hold that mind is more potent than matter, and nearer to the real essence of things. Our science is in any case necessarily dualistic, being the product of the reaction of mind on nature, and must be largely subjective and anthropomorphic. Hence, no doubt, arise much of the controversy of science, and much of the unsolved difficulty. We recognize this when we divide science into that which is experimental, or depends on apparatus, and that which is observational and classificatory, — distinctions, these, which relate not so much to the objects of science as to our methods of pursuing them. This view also opens up to us the thought that the domain of science is practically boundless; for who can set limits to the action of mind on the universe, or of the universe on mind? It follows that science must be limited on all sides by unsolved mysteries; and it will not serve any good purpose to meet these with clever guesses. If we so treat the enigmas of the sphinx nature, we shall surely be devoured. Nor, on the other hand, must we collapse into absolute despair, and resign ourselves to the confession of inevitable ignorance. It becomes us, rather, boldly to confront the unsolved questions of nature, and to wrestle with their difficulties till we master such as we can, and cheerfully leave those we cannot overcome to be grappled with by our successors.

Fortunately, as a geologist, I do not need to invite your attention to those transcendental questions which relate to the ultimate constitution of matter, the nature of the ethereal medium filling space, the absolute difference or identity of chemical elements, the cause of gravitation, the conservation and dissipation of energy, the nature of life, or the primary origin of bioplasmic matter. I may take the much more humble *rôle* of an inquirer into the unsolved or partially solved problems which meet us in considering that short and imperfect record which geology studies in the rocky layers of the earth's crust, and which leads no farther back than to the time when a solid rind had already formed on the earth and was already covered with an ocean. This record of geology covers but a small part of the history of the earth and of the system to which it belongs, nor does it enter at all into the more recondite problems involved; still it forms, I believe, some necessary preparation, at least, to the comprehension of these.

What do we know of the oldest and most primitive rocks? At this moment the question may be answered in many and discordant ways; yet the leading elements of the answer may be given very simply. The oldest rock formation known to geologists is the lower Laurentian, the fundamental gneiss, the Lewisian formation of Scotland, the Ottawa gneiss of Canada. This formation of enormous thickness corresponds to what the older geologists called the fundamental granite, — a name not to be scouted, for gneiss is only a stratified granite. Perhaps the main fact in relation to this old rock is that it is a gneiss; that is, a rock at once bedded and crystalline, and having for its dominant ingredient the mineral orthoclase, — a compound of silica, alumina, and potash, — in which are embedded, as in a paste, grains and crystals of quartz and hornblende. We know very well, from its texture and composition, that it cannot be a product of mere heat; and, being a bedded rock, we infer that it was laid down layer by layer, in the manner of aqueous deposits. On the other hand, its chemical composition is quite different from that of the muds, sands, and gravels usually deposited from water. Their special characters are caused by the fact that they have resulted from the slow decay of rocks like these gneisses, under the operation of carbonic acid and water, whereby the alkaline matter and the more soluble part of the silica have been washed away, leaving a residue mainly siliceous and aluminous. Such more modern rocks tell of dry land subjected to atmospheric decay and rainwash. If they have any direct relation to the old gneisses, they are their grandchildren, not their parents. On the contrary, the oldest gneisses show no pebbles, or sand, or limestone — nothing to indicate that there was then any land undergoing atmospheric waste, or shores with sand and gravel. For all that we know to the contrary, these old gneisses may have been deposited in a shoreless sea, holding in solution or suspension merely what it could derive from a submerged crust recently cooled from a state of fusion, still thin, and exuding here and there through its fissures heated waters and volcanic products.

It is scarcely necessary to say that I have no confidence in the supposition of unlike composition of the earth's mass on different sides, on which Dana has partly based his theory of the origin of continents. The most probable conception seems to be that of Lyell; namely, a molten mass, uniform except in so far as denser material might exist toward its centre, and a crust at first approximately even and homogeneous, and subsequently thrown into great bendings upward and downward. This question has recently been ably discussed by Mr. Crosby in the London *Geological magazine*.[1]

In short, the fundamental gneiss of the lower Laurentian may have been the first rock ever formed; and in any case it is a rock formed under conditions which have not since recurred, except locally. It constitutes the first and best example of these chemico-physical, aqueous or aqueo-igneous rocks, so characteristic of the earliest period of the earth's history. Viewed in this way, the lower Laurentian gneiss is probably the oldest kind of rock we shall ever know, — the limit to our backward progress, beyond which there remains nothing to the geologist, except physical hypotheses respecting a cooling, incandescent globe. For the chemical conditions of these primitive rocks, and what is known as to their probable origin, I must refer you to my friend Dr. Sterry Hunt, to whom we owe so much of what is known of the older crystalline rocks,[2] as well as of their literature and the questions which they raise. My purpose here is to sketch the remarkable difference which we meet as we ascend into the middle and upper Laurentian.

[1] June, 1883. [2] Hunt, Essays on chemical geology.

In the next succeeding formation, the true lower Laurentian of Logan, the Grenville series of Canada, we meet with a great and significant change. It is true, we have still a predominance of gneisses which may have been formed in the same manner with those below them; but we find these now associated with great beds of limestone and dolomite, which must have been formed by the separation of calcium and magnesium carbonates from the sea-water, either by chemical precipitation or by the agency of living beings. We have also quartzite, quartzose gneisses, and even pebble beds, which inform us of sand-banks and shores. Nay, more, we have beds containing graphite which must be the residue of plants, and iron ores which tell of the deoxidation of iron oxide by organic matters. In short, here we have evidence of new factors in world-building, — of land and ocean, of atmospheric decay of rocks, of deoxidizing processes carried on by vegetable life on the land and in the waters, of limestone-building in the sea. To afford material for such rocks, the old Ottawa gneiss must have been lifted up into continents and mountain masses. Under the slow but sure action of the carbonic dioxide dissolved in rain-water, its felspar had crumbled down in the course of ages. Its potash, soda, lime, magnesia, and part of its silica, had been washed into the sea, there to enter into new combinations, and to form new deposits. The crumbling residue of fine clay and sand had been also washed down into the borders of the ocean, and had been there deposited in beds.[1] Thus the earth had entered into a new phase, which continues onward through the geological ages; and I place in your hands one key for unlocking the mystery of the world when I affirm that this great change took place, this new era was inaugurated, in the midst of the Laurentian period.

Was not this time a fit period for the first appearance of life? Should we not expect it to appear, independently of the evidence we have of the fact? I do not propose to enter here into that evidence, more especially in the case of the one well characterized Laurentian fossil, Eozoon canadense. I have already amply illustrated it elsewhere. I would merely say here, that we should bear in mind that in this later half of the lower Laurentian, or, if we so choose to style it, middle Laurentian period, we have the conditions required for life in the sea and on the land; and, since in other periods we know that life was always present when its conditions were present, it is not unreasonable to look for the first traces of life in this formation, in which we find for the first time the completion of those physical arrangements which make life, in such forms of it as exist on our planet, possible.

This is also a proper place to say something of the doctrine of what is termed 'metamorphism.' The Laurentian rocks are undoubtedly greatly changed from their original state, more especially in the matters of crystallization and the formation of desseminated minerals by the action of heat and heated water. Sandstones have thus passed into quartzites, clays into slates and schists, limestones into marbles. So far, metamorphism is not a doubtful question; but, when theories of metamorphism go so far as to suppose an actual change of one element for another, they go beyond the bounds of chemical credibility; yet such theories of metamorphism are often boldly advanced, and made the basis of important conclusions. Dr. Hunt has happily given the name 'metasomatosis' to this imaginary and impossible kind of metamorphism, which may be regarded as an extreme kind of evolution, akin to some of those forms of that theory employed with reference to life, but more easily detected and exposed. I would have it to be understood, that, in speaking of the metamorphism of the older crystalline rocks, it is not to this metasomatosis that I refer, and that I hold that rocks which have been produced out of the materials decomposed by atmospheric erosion can never, by any process of metamorphism, be restored to the precise condition of the Laurentian rocks. Thus there is in the older formations a genealogy of rocks, which, in the absence of fossils, may be used with some confidence, but which does not apply to the more modern deposits. Still, nothing in geology absolutely perishes or is altogether discontinued; and it is probable, that, down to the present day, the causes which produced the old Laurentian gneiss may still operate in limited localities. Then, however, they were general, not exceptional. It is further to be observed, that the term 'gneiss' is sometimes of wide and even loose application. Beside the typical orthoclase and hornblendic gneiss of the Laurentian, there are micaceous, quartzose, garnetiferous, and many other kinds of gneiss; and even gneissose rocks, which hold labradorite or anorthite instead of orthoclase, are sometimes, though not accurately, included in the term.

The Grenville series, or middle Laurentian, is succeeded by what Logan in Canada called the upper Laurentian, and which other geologists have called the Norite or Norian series. Here we still have our old friends the gneisses, but somewhat peculiar in type; and associated with them are great beds rich in lime-felspar, — the so-called labradorite and anorthite rocks. The precise origin of these is uncertain, but this much seems clear; namely, that they originated in circumstances in which the great limestones deposited in the lower or middle Laurentian were beginning to be employed in the manufacture, probably by aqueo-igneous agencies, of lime-felspars. This proves the Norian rocks to be much younger than the Laurentian, and that, as Logan supposed, considerable earth-movements had occurred between the two, implying lapse of time.

Next we have the Huronian of Logan, — a series much less crystalline and more fragmentary, and affording more evidence of land elevation and atmospheric and aqueous erosion, than any of the others. It has great conglomerates, some of them made up of rounded pebbles of Laurentian rocks, and others of quartz pebbles, which must have been the remains of rocks subjected to very perfect erosion. The pure

[1] Dr. Hunt has now in preparation for the press an important paper on this subject, read before the National academy of sciences.

quartz rocks tell the same tale, while limestones and slates speak also of chemical separation of the materials of older rocks. The Huronian evidently tells of movements in the previous Laurentian, and changes in its texture so great, that the former may be regarded as a comparatively modern rock, though vastly older than any part of the paleozoic series.

Still later than the Huronian is the great micaceous series called by Hunt the Mont Alban or White Mountain group, and the Taconian or lower Taconic of Emmons, which recalls in some measure the conditions of the Huronian. The precise relations of these to the later formations, and to certain doubtful deposits around Lake Superior, can scarcely be said to be settled, though it would seem that they are all older than the fossiliferous Cambrian rocks which practically constitute the base of the paleozoic. I have, I may say, satisfied myself, in regions which I have studied, of the existence and order of these rocks as successive formations, though I would not dogmatize as to the precise relations of those last mentioned, or as to the precise age of some disputed formations which may either be of the age of the older eozoic formations, or may be peculiar kinds of paleozoic rocks modified by metamorphism. Probably neither of the extreme views now agitated is absolutely correct.

After what has been said, you will perhaps not be astonished that a great geological battle rages over the old crystalline rocks. By some geologists they are almost entirely explained away, or referred to igneous action or to the alteration of ordinary sediments. Under the treatment of another school, they grow to great series of pre-Cambrian rocks, constituting vast systems of formations, distinguishable from each other, not by fossils, but by differences of mineral character. I have already indicated the manner in which I believe the dispute will ultimately be settled, and the president of the geological section will treat it more fully in his opening address.

After the solitary appearance of Eozoon in the Laurentian, and of a few uncertain forms in the Huronian and Taconian, we find ourselves in the Cambrian, in the presence of a nearly complete invertebrate fauna of protozoa, polyps, echinoderms, mollusks, and crustacea; and this not confined to one locality merely, but apparently extended simultaneously throughout the ocean. This sudden incoming of animal life, along with the subsequent introduction of successive groups of invertebrates, and finally of vertebrate animals, furnishes one of the greatest of the unsolved problems of geology, which geologists were wont to settle by the supposition of successive creations. In an address delivered at the Detroit meeting of the association in 1875, I endeavored to set forth the facts as to this succession, and the general principles involved in it, and to show the insufficiency of the theories of evolution suggested by biologists to give any substantial aid to the geologist in these questions. In looking again at the points there set forth, I find they have not been invalidated by subsequent discoveries, and that we are still nearly in the same position with respect to these great questions that we were in at that time, — a singular proof of the impotency of that deductive method of reasoning which has become fashionable among naturalists of late. Yet the discussions of recent years have thrown some additional light on these matters; and none more so than the mild disclaimers with which my friend Dr. Asa Gray and other moderate and scientific evolutionists have met the extreme views of such men as Romanes, Haeckel, Lubbock, and Grant Allen. It may be useful to note some of these as shedding a little light on this dark corner of our unsolved problems.

It has been urged on the side of rational evolution, that this hypothesis does not profess to give an explanation of the absolute origin of life on our planet, or even of the original organization of a single cell or of a simple mass of protoplasm, living or dead. All experimental attempts to produce by synthesis the complex albuminous substances, or to obtain the living from the non-living, have so far been fruitless; and, indeed, we cannot imagine any process by which such changes could be effected. That they have been effected we know; but the process employed by their maker is still as mysterious to us as it probably was to him who wrote the words, 'And God said let the waters swarm with swarmers.' How vast is the gap in our knowledge and our practical power implied in this admission, which must, however, be made by every mind not absolutely blinded by a superstitious belief in those forms of words which too often pass current as philosophy!

But if we are content to start with a number of organisms ready made, — a somewhat humiliating start, however, — we still have to ask, How do these vary so as to give new species? It is a singular illusion in this matter, of men who profess to be believers in natural law, that variation may be boundless, aimless, and fortuitous, and that it is by spontaneous selection from varieties thus produced that development arises. But surely the supposition of mere chance and magic is unworthy of science. Varieties must have causes, and their causes and their effects must be regulated by some law or laws. Now, it is easy to see that they cannot be caused by a mere innate tendency in the organism itself. Every organism is so nicely equilibrated, that it has no such spontaneous tendency, except within the limits set by its growth and the law of its periodical changes. There may, however, be equilibrium more or less stable. I believe all attempts hitherto made have failed to account for the fixity of certain, nay, of very many, types throughout geological time; but the mere consideration that one may be in a more stable state of equilibrium than another so far explains it. A rocking stone has no more spontaneous tendency to move than an ordinary bowlder, but it may be made to move with a touch. So it probably is with organisms. But, if so, then the causes of variation are external, as in many cases we actually know them to be; and they must depend on instability or change in surroundings, and this so arranged as not to be too extreme in amount, and to operate in some determinate direction. Observe how remarkable the unity

of the adjustments involved in such a supposition. How superior they must be to our rude and always more or less unsuccessful attempts to produce and carry forward varieties and races in definite directions! This cannot be chance. If it exists, it must depend on plans deeply laid in the nature of things, else it would be most monstrous magic and causeless miracle. Still more certain is this conclusion when we consider the vast and orderly succession made known to us by geology, and which must have been regulated by fixed laws, only a few of which are as yet known to us.

Beyond these general considerations, we have others of a more special character, based on paleontological facts, which show how imperfect are our attempts, as yet, to reach the true causes of the introduction of genera and species.

One is the remarkable fixity of the leading types of living beings in geological time. If instead of framing, like Haeckel, fanciful phylogenies, we take the trouble, with Barrande and Gaudry, to trace the forms of life through the period of their existence, each along its own line, we shall be greatly struck with this, and especially with the continuous existence of many low types of life through vicissitudes of physical conditions of the most stupendous character, and over a lapse of time scarcely conceivable. What is still more remarkable is, that this holds in groups which, within certain limits, are perhaps the most variable of all. In the present world no creatures are individually more variable than the protozoa; as, for example, the foraminifera and the sponges. Yet these groups are fundamentally the same, from the beginning of the palaeozoic until now; and modern species seem scarcely at all to differ from specimens procured from rocks at least half-way back to the beginning of our geological record. If we suppose that the present sponges and foraminifera are the descendants of those of the Silurian period, we can affirm, that, in all that vast lapse of time, they have, on the whole, made little greater change than that which may be observed in variable forms at present. The same remark applies to other low animal forms. In forms somewhat higher and less variable, this is equally noteworthy. The pattern of the venation of the wings of cockroaches, and the structure and form of land-snails, gally-worms, and decapod crustaceans, were all settled in the carboniferous age in a way that still remains. So were the foliage and the fructification of club-mosses and ferns. If at any time members of these groups branched off, so as to lay the foundation of new species, this must have been a very rare and exceptional occurrence, and one demanding even some suspension of the ordinary laws of nature.

Certain recent utterances of eminent scientific men in England and France are most instructive with reference to the difficulties which encompass this subject. Huxley, at present the leader of English evolutionists, in his 'Rede lecture'[1] delivered at Cambridge, England, holds that there are only two 'possible alternative hypotheses' as to the origin of species, — (1) that of 'construction,' or the mechanical putting-together of the materials and parts of each new species separately; and (2) that of 'evolution,' or that one form of life 'proceeded from another' by the 'establishment of small successive differences.' After comparing these modes, much to the disadvantage of the first, he concludes with the statement that "this was his case for evolution, which he rested wholly on arguments of the kind he had adduced;" these arguments being the threadbare false analogy of ordinary reproduction and the transformation of species, and the mere succession of forms more or less similar in geological time, neither of them having any bearing whatever on the origin of any species or on the cause of the observed succession. With reference to the two alternatives, while it is true that no certain evidence has yet been obtained — either by experiment, observation, or sound induction — as to the mode of origin of any species, enough is known to show that there are numerous possible methods, grouped usually under the heads of absolute creation, mediate creation, critical evolution, and gradual evolution. It is also true that almost the only thing we certainly know in the matter, is that the differences characteristic of classes, orders, genera, and species, must have arisen, not in one or two, but in many ways. An instructive commentary on the capacity of our age to deal with these great questions is afforded by the fact that this little piece of clever mental gymnastic should have been practised in a university lecture and in presence of an educated audience. It is also deserving of notice, that, though the lecturer takes the development of the Nautili and their allies as his principal illustration, he evidently attaches no weight to the argument in the opposite sense deduced by Barrande — the man of all others most profoundly acquainted with these animals — from the paleozoic cephalopods.

Another example is afforded by a lecture recently delivered at the Royal institution in London by Professor Flower.[1] The subject is, 'The whales, past and present, and their probable origin.' The latter point, as is well known, Gaudry had candidly given up. "We have questioned," he says, "these strange and gigantic sovereigns of the tertiary oceans as to their ancestors, — they leave us without reply." Flower is bold enough to face this problem; and he does so in a fair and vigorous way, though limiting himself to the supposition of slow and gradual change. He gives up at once, as every anatomist must, the idea of an origin from fishes or reptiles. He thinks the ancestors of the whales must have been quadrupedal mammals. He is obliged for good reasons to reject the seals and the otters, and turns to the ungulates, though here, also, the difficulties are formidable. Finally he has recourse to an imaginary ancestor, supposed to have haunted marshes and rivers of the mesozoic age, and to have been intermediate between a hippopotamus and a dolphin, and omnivorous in diet. As this animal is altogether unknown to geology or zoölogy, and not much less difficult to account for than the whales themselves,

[1] Report in *Nature*, June 21, corrected by the author.

[1] Reported in *Nature*.

he very properly adds, 'Please to recollect, however, that this is a mere speculation.' He trusts, however, that such speculations are 'not without their use;' but this will depend upon whether or not they lead men's minds from the path of legitimate science into the quicksands of baseless conjecture.

Gaudry, in his recent work, 'Enchainements du monde animal,' [1] though a strong advocate of evolution, is obliged in his final *résumé* to say, "Il ne laisse point percer le mystère qui entoure le developpement primitif des grandes classes du monde animal. Nul homme ne sait comment ont été formés les premiers individus de foraminifères, de polypes, d'étoiles de mer, de crinoides, etc. Les fossiles primaires ne nous ont pas encore fourni de preuves positives du passage des animaux d'une classe à ceux d'une autre classe."

Professor Williamson of Manchester, in an address delivered in February last before the Royal institution of Great Britain, after showing that the conifers, ferns, and lycopods of the paleozoic have no known ancestry, uses the significant words, "The time has not yet arrived for the appointment of a botanical king-at-arms and constructor of pedigrees."

Another caution which a paleontologist has occasion to give with regard to theories of life has reference to the tendency of biologists to infer that animals and plants were introduced under embryonic forms, and at first in few and imperfect species. Facts do not substantiate this. The first appearance of leading types of life is rarely embryonic. On the contrary, they often appear in highly perfect and specialized forms; often, however, of composite type, and expressing characters afterwards so separated as to belong to higher groups. The trilobites of the Cambrian are some of them of few segments, and, so far, embryonic; but the greater part are many-segmented and very complex. The batrachians of the carboniferous present many characters higher than those of their modern successors, and now appropriated to the true reptiles. The reptiles of the Permian and trias usurped some of the prerogatives of the mammals. The ferns, lycopods, and equisetums of the Devonian and carboniferous were, to say the least, not inferior to their modern representatives. The shell-bearing cephalopods of the paleozoic would seem to have possessed structures now special to a higher group, that of the cuttle-fishes. The bald and contemptuous negation of these facts by Haeckel and other biologists does not tend to give geologists much confidence in their dicta.

Again: we are now prepared to say that the struggle for existence, however plausible as a theory, when put before us in connection with the productiveness of animals, and the few survivors of their multitudinous progeny, has not been the determining cause of the introduction of new species. The periods of rapid introduction of new forms of marine life were not periods of struggle, but of expansion, — those periods in which the submergence of continents afforded new and large space for their extension and comfortable subsistence. In like manner it was continental emergence that afforded the opportunity for the introduction of land animals and plants. Further, in connection with this, it is now an established conclusion, that the great aggressive faunas and floras of the continents have originated in the north, some of them within the arctic circle; and this in periods of exceptional warmth, when the perpetual summer sunshine of the arctic regions co-existed with a warm temperature. The testimony of the rocks thus is, that not struggle, but expansion, furnished the requisite conditions for new forms of life, and that the periods of struggle were characterized by depauperation and extinction.

But we are sometimes told that organisms are merely mechanical, and that the discussions respecting their origin have no significance, any more than if they related to rocks or crystals, because they relate merely to the organism considered as a machine, and not to that which may be supposed to be more important; namely, the great determining power of mind and will. That this is a mere evasion, by which we really gain nothing, will appear from a characteristic extract of an article by an eminent biologist, in the new edition of the Encyclopedia Britannica, — a publication which, I am sorry to say, instead of its proper *rôle* as a repertory of facts, has become a strong partisan, stating extreme and unproved speculations as if they were conclusions of science. The statement referred to is as follows: "A mass of living protoplasm is simply a molecular machine of great complexity, the total results of the working of which, or its vital phenomena, depend on the one hand on its construction, and, on the other, on the energy supplied to it; and to speak of vitality as any thing but the name for a series of operations is as if one should talk of the horologity of a clock." It would, I think, scarcely be possible to put into the same number of words a greater amount of unscientific assumption and unproved statement than in this sentence. Is 'living protoplasm' different in any way from dead protoplasm, and, if so, what causes the difference? What is a 'machine'? Can we conceive of a self-produced or uncaused machine, or one not intended to work out some definite results? The results of the machine in question are said to be 'vital phenomena;' certainly most wonderful results, and greater than those of any machine man has yet been able to construct. But why 'vital'? If there is no such thing as life, surely they are merely physical results. Can mechanical causes produce other than physical effects? To Aristotle, life was 'the cause of form in organisms.' Is not this quite as likely to be true as the converse proposition? If the vital phenomena depend on the 'construction' of the machine, and the 'energy supplied to it,' whence this construction, and whence this energy? The illustration of the clock does not help us to answer this question. The construction of the clock depends on its maker, and its energy is derived from the hand that winds it up. If we can think of a clock which no one has made and which no one winds, — a clock constructed by chance, set in harmony with the universe by chance,

[1] Paris, 1883.

wound up periodically by chance, — we shall then have an idea parallel to that of an organism living, yet without any vital energy or creative law; but in such a case we should certainly have to assume some antecedent cause, whether we call it 'horologity' or by some other name. Perhaps the term 'evolution' would serve as well as any other, were it not that common sense teaches that nothing can be spontaneously evolved out of that in which it did not previously exist.

There is one other unsolved problem, in the study of life by the geologist, to which it is still necessary to advert. This is the inability of paleontology to fill up the gaps in the chain of being. In this respect, we are constantly taunted with the imperfection of the record; but facts show that this is much more complete than is generally supposed. Over long periods of time and many lines of being, we have a nearly continuous chain; and, if this does not show the tendency desired, the fault is as likely to be in the theory as in the record. On the other hand, the abrupt and simultaneous appearance of new types in many specific and generic forms, and over wide and separate areas at one and the same time, is too often repeated to be accidental. Hence paleontologists, in endeavoring to establish evolution, have been obliged to assume periods of exceptional activity in the introduction of species, alternating with others of stagnation, — a doctrine differing very little from that of special creation as held by the older geologists.

The attempt has lately been made to account for these breaks by the assumption that the geological record relates only to periods of submergence, and gives no information as to those of elevation. This is manifestly untrue. In so far as marine life is concerned, the periods of submergence are those in which new forms abound for very obvious reasons already hinted. But the periods of new forms of land and fresh-water life are those of elevation, and these have their own records and monuments, often very rich and ample; as, for example, the swamps of the carboniferous, the transition from the cretaceous subsidence to the Laramie elevation, the tertiary lake-basins of the west, the terraces and raised beaches of the pleistocene. Had I time to refer in detail to the breaks in the continuity of life, which cannot be explained by the imperfection of the record, I could show at least that nature, in this case, does advance *per saltum*, — by leaps, rather than by a slow continuous process. Many able reasoners, as LeConte in this country, and Mivart and Collard in England, hold this view.

Here, as elsewhere, a vast amount of steady conscientious work is required to enable us to solve the problems of the history of life. But, if so, the more the hope for the patient student and investigator. I know nothing more chilling to research, or unfavorable to progress, than the promulgation of a dogmatic decision that there is nothing to be learned but a merely fortuitous and uncaused succession, amenable to no law, and only to be covered, in order to hide its shapeless and uncertain proportions, by the mantle of bold and gratuitous hypothesis.

So soon as we find evidence of continents and oceans, we raise the question, "Have these continents existed from the first in their present position and form, or have the land and water changed places in the course of geological time?" In reality both statements are true in a certain limited sense. On the one hand, any geological map whatever suffices to show that the general outline of the existing land began to be formed in the first and oldest crumplings of the crust. On the other hand, the greater part of the surface of the land consists of marine sediments which must have been derived from land that has perished in the process, while all the continental surfaces, except, perhaps, some high peaks and ridges, have been many times submerged. Both of these apparently contradictory statements are true; and, without assuming both, it is impossible to explain the existing contours and reliefs of the surface.

In the case of North America, the form of the old nucleus of Laurentian rock in the north already marks out that of the finished continent, and the successive later formations have been laid upon the edges of this, like the successive loads of earth dumped over an embankment. But in order to give the great thickness of the paleozoic sediments, the land must have been again and again submerged, and for long periods of time. Thus, in one sense, the continents have been fixed; in another, they have been constantly fluctuating. Hall and Dana have well illustrated these points in so far as eastern North America is concerned. Professor Hull of the Geological survey of Ireland has recently had the boldness to reduce the fluctuations of land and water, as evidenced in the British Islands, to the form of a series of maps intended to show the physical geography of each successive period. The attempt is probably premature, and has been met with much adverse criticism; but there can be no doubt that it has an element of truth. When we attempt to calculate what could have been supplied from the old eozoic nucleus by decay and aqueous erosion, and when we take into account the greater local thickness of sediments towards the present sea-basins, we can scarcely avoid the conclusion that extensive areas once occupied by high land are now under the sea. But to ascertain the precise areas and position of these perished lands may now be impossible.

In point of fact, we are obliged to believe in the contemporaneous existence in all geological periods, except perhaps the very oldest, of three sorts of areas on the surface of the earth: 1. Oceanic areas of deep sea, which must always have occupied the bed of the present ocean, or parts of it; 2. Continental plateaus, sometimes existing as low flats or as higher tablelands, and sometimes submerged; 3. Areas of plication or folding, more especially along the borders of the oceans, forming elevated lands rarely submerged, and constantly affording the material of sedimentary accumulations.

Every geologist knows the contention which has been occasioned by the attempts to correlate the earlier paleozoic deposits of the Atlantic margin of North America with those forming at the same time

on the interior plateau, and with those of intervening lines of plication and igneous disturbance. Stratigraphy, lithology, and fossils are all more or less at fault in dealing with these questions; and, while the general nature of the problem is understood by many geologists, its solution in particular cases is still a source of apparently endless debate.

The causes and mode of operation of the great movements of the earth's crust which have produced mountains, plains, and tablelands, are still involved in some mystery. One patent cause is the unequal settling of the crust toward the centre; but it is not so generally understood as it should be, that the greater settlement of the ocean-bed has necessitated its pressure against the sides of the continents in the same manner that a huge ice-floe crushes a ship or a pier. The geological map of North America shows this at a glance, and impresses us with the fact that large portions of the earth's crust have not only been folded, but bodily pushed back for great distances. On looking at the extreme north, we see that the great Laurentian mass of central Newfoundland has acted as a protecting pier to the space immediately west of it, and has caused the Gulf of St. Lawrence to remain an undisturbed area since paleozoic times. Immediately to the south of this, Nova Scotia and New Brunswick are folded back. Still farther south, as Guyot has shown, the old sediments have been crushed in sharp folds against the Adirondack mass, which has sheltered the tableland of the Catskills and of the Great Lakes. South of this again, the rocks of Pennsylvania and Maryland have been driven back in a great curve to the west. Nothing, I think, can more forcibly show the enormous pressure to which the edges of the continents have been exposed, and at the same time the great sinking of the ocean-beds. Complex and difficult to calculate though these movements of plication are, they are more intelligible than the apparently regular pulsations of the flat continental areas, whereby they have alternately been below and above the waters, and which must have depended on somewhat regularly recurring causes, connected either with the secular cooling of the earth, or with the gradual retardation of its rotation, or with both. Throughout these changes, each successive elevation exposed the rocks for long ages to the decomposing influence of the atmosphere. Each submergence swept away, and deposited as sediment, the material accumulated by decay. Every change of elevation was accompanied with changes of climate and with modifications of the habitats of animals and plants. Were it possible to restore accurately the physical geography of the earth in all these respects, for each geological period, the data for the solution of many difficult questions would be furnished.

It is an unfortunate circumstance, that conclusions in geology arrived at by the most careful observation and induction do not remain undisturbed, but require constant vigilance to prevent them from being overthrown. Sometimes, of course, this arises from new discoveries throwing new light on old facts; but when this occurs it rarely works the complete subversion of previously received views. The more usual case is, that some over-zealous specialist suddenly discovers what seems to him to overturn all previous beliefs, and rushes into print with a new and plausible theory, which at once carries with him a host of half-informed people, but the insufficiency of which is speedily made manifest.

Had I written this address a few years ago, I might have referred to the mode of formation of coal as one of the things most surely settled and understood. The labors of many eminent geologists, microscopists, and chemists in the old and the new worlds had shown that coal nearly always rests upon old soil surfaces penetrated with roots, and that coal-beds have in their roofs erect trees, the remains of the last forests that grew upon them. Logan and I have illustrated this in the case of the series of more than sixty successive coal-beds exposed at the South Joggins, and have shown unequivocal evidence of land-surfaces at the time of the deposition of the coal. Microscopical examination has proved that these coals are composed of the materials of the same trees whose roots are found in the underclays, and their stems and leaves in the roof-shales; that much of the material of the coal has been subjected to sub-aerial decay at the time of its accumulation; and that in this, ordinary coal differs from bituminous shale, earthy bitumen, and some kinds of cannel, which have been formed under water; that the matter remaining as coal consists almost entirely of epidermal tissues, which, being suberose in character, are highly carbonaceous, very durable, and impermeable by water,[1] and are hence the best fitted for the production of pure coal; and finally that the vegetation and the climatal and geographical features of the coal period were eminently fitted to produce in the vast swamps of that period precisely the effects observed. All these points and many others have been thoroughly worked out for both European and American coal-fields, and seemed to leave no doubt on the subject. But several years ago certain microscopists observed on slices of coal layers filled with spore-cases, — a not unusual circumstance, since these were shed in vast abundance by the trees of the coal-forests, and because they contain suberose matter of the same character with epidermal tissues generally. Immediately we were informed that all coal consists of spores; and, this being at once accepted by the unthinking, the results of the labors of many years are thrown aside in favor of this crude and partial theory. A little later, a German microscopist has thought proper to describe coal as made up of minute algae, and tries to reconcile this view with the appearances, devising at the same time a new and formidable nomenclature of generic and specific names, which would seem largely to represent mere fragments of tissues. Still later, some local facts in a French coal-field have induced an eminent botanist of that country to revive the drift theory of coal, in opposition to that of growth *in situ*. A year or two ago, when my friend Professor Williamson of Manchester informed me that he was preparing a large series of slices of coal with the view of revis-

[1] Acadian geology, third edition, supplement, p. 68.

ing the whole subject, I was inclined to say, that after what had been done by Lyell, Goeppert, Logan, Hunt, Newberry, and myself, this was scarcely necessary; but, in view of what I have just stated, it may be that all he can do will be required to rescue from total ruin the results of our labors.

An illustration of a different character is afforded by the controversy now raging with respect to the so-called fucoids of the ancient rocks. At one time the group of fucoids, or algae, constituted a general place of refuge for all sorts of unintelligible forms and markings; graptolites, worm-trails, crustacean tracks, shrinkage-cracks, and, above all, rill-markings, forming a heterogeneous group of fucoidal remains distinguished by generic and specific names. To these were also added some true land-plants badly preserved, or exhibiting structures not well understood by botanists. Such a group was sure to be eventually dismembered. The writer has himself done something toward this,[1] but Professor Nathorst has done still more;[2] and now some intelligible explanation can be given of many of these forms. Quite recently, however, the Count de Saporta, in an elaborate illustrated memoir,[3] has come to the defence of the fucoids, more especially against the destructive experiments of Nathorst, and would carry back into the vegetable kingdom many things which would seem to be mere trails of animals. While writing this address, I have received from Professor Crié of Rennes a paper in which he not only supports the algal nature of Rusichnites, Arthrichnites, and many other supposed fucoids, but claims for the vegetable kingdom even Receptaculites and Archaeocyathus. It is not to be denied that some of the facts which he cites, respecting the structure of the Siphoniae and of certain modern incrusting algae, are very suggestive, though I cannot agree with his conclusions. My own experience has convinced me, that, while non-botanical geologists are prone to mistake all kinds of markings for plants, even good botanists, when not familiar with the chemical and mechanical conditions of fossilization, and with the present phenomena of tidal shores, are quite as easily misled, though they are very prone, on the other hand, to regard land-plants of some complexity, when badly preserved, as mere algae. In these circumstances it is very difficult to secure any consensus, and the truth is only to be found by careful observation of competent men. One trouble is, that these usually obscure markings have been despised by the greater number of paleontologists, and probably would not now be so much in controversy were it not for the use made of them in illustrating supposed phylogenies of plants.

It would be wrong to close this address without some reference to that which is the veritable *pons asinorum* of the science, the great and much debated glacial period. I trust that you will not suppose, that, in the end of an hour's address, I am about to discuss this vexed question. Time would fail me even to name the hosts of recent authors who have contended in this arena. I can hope only to point out a few landmarks which may aid the geological adventurer in traversing the slippery and treacherous surface of the hypothetical ice-sheet of pleistocene times, and in avoiding the yawning crevasses by which it is traversed.

No conclusions of geology seem more certain than that great changes of climate have occurred in the course of geological time; and the evidence of this in that comparatively modern period which immediately preceded the human age is so striking that it has come to be known as pre-eminently the ice age, while, in the preceding tertiary periods, temperate conditions seem to have prevailed even to the pole. Of the many theories as to these changes which have been proposed, two seem at present to divide the suffrages of geologists, either alone, or combined with each other. These are, (1) the theory of the precession of the equinoxes in connection with the varying eccentricity of the earth's orbit, advocated more especially by Croll; and (2) the different distribution of land and water as affecting the reception and radiation of heat and the ocean-currents, — a theory ably propounded by Lyell, and subsequently extensively adopted, either alone or with the previous one. One of these views may be called the astronomical; the other, the geographical. I confess that I am inclined to accept the second or Lyellian theory for such reasons as the following: 1. Great elevations and depressions of land have occurred in and since the pleistocene, while the alleged astronomical changes are not certain, more especially in regard to their probable effect on the earth; 2. When the rival theories are tested by the present phenomena of the southern polar region and the North Atlantic, there seem to be geographical causes adequate to account for all except extreme and unproved glacial conditions; 3. The astronomical cause would suppose regularly recurring glacial periods of which there is no evidence, and it would give to the latest glacial age an antiquity which seems at variance with all other facts; 4. In those more northern regions where glacial phenomena are most pronounced, the theory of floating sheets of ice, with local glaciers descending to the sea, seems to meet all the conditions of the case; and these would be obtained, in the North Atlantic at least, by very moderate changes of level, causing, for example, the equatorial current to flow into the Pacific, instead of running northward as a gulf stream; 5. The geographical theory allows the supposition not merely of vicissitudes of climate quickly following each other in unison with the movements of the surface, but allows also of that near local approximation of regions wholly covered with ice and snow, and others comparatively temperate, which we see at present in the north.

If, however, we are to adopt the geographical theory, we must avoid extreme views; and this leads to the inquiry as to the evidence to be found for any such universal and extreme glaciation as is demanded by some geologists.

[1] Footprints and impressions on carboniferous rocks, *Amer. ourn. sc.*, 1873.

[2] Royal Swedish academy, Stockholm, 1881.

[3] Apropos des algues fossiles, Paris, 1883.

The only large continental area in the northern hemisphere supposed to be entirely ice- and snow-clad is Greenland; and this, so far as it goes, is certainly a local case, for the ice and snow of Greenland extend to the south as far as 60° N. latitude, while both in Norway and in the interior of North America the climate in that latitude permits the growth of cereals. Further, Grinnel Land, which is separated from North Greenland only by a narrow sound, has a comparatively mild climate, and, as Nares has shown, is covered with verdure in summer. Still further, Nordenskiöld, one of the most experienced arctic explorers, holds that it is probable that the interior of Greenland is itself verdant in summer, and is at this moment preparing to attempt to reach this interior oasis. Nor is it difficult, with the aid of the facts cited by Woeickoff and Whitney,[1] to perceive the cause of the exceptional condition of Greenland. To give ice and snow in large quantities, two conditions are required, — first, atmospheric humidity; and, secondly, cold precipitating regions. Both of these conditions meet in Greenland. Its high coast-ranges receive and condense the humidity from the sea on both sides of it and to the south. Hence the vast accumulation of its coast snow-fields, and the intense discharge of the glaciers emptying out of its valleys. When extreme glacialists point to Greenland, and ask us to believe that in the glacial age the whole continent of North America as far south as the latitude of 40° was covered with a continental glacier, in some places several thousands of feet thick, we may well ask, first, what evidence there is that Greenland, or even the antarctic continent, at present shows such a condition; and, secondly, whether there exists a possibility that the interior of a great continent could ever receive so large an amount of precipitation as that required. So far as present knowledge exists, it is certain that the meteorologist and the physicist must answer both questions in the negative. In short, perpetual snow and glaciers must be local, and cannot be continental, because of the vast amount of evaporation and condensation required. These can only be possible where comparatively warm seas supply moisture to cold and elevated land; and this supply cannot, in the nature of things, penetrate far inland. The actual condition of interior Asia and interior America in the higher northern latitudes affords positive proof of this. In a state of partial submergence of our northern continents, we can readily imagine glaciation by the combined action of local glaciers and great ice-floes; but, in whatever way the phenomena of the bowlder clay and of the so-called terminal moraines are to be accounted for, the theory of a continuous continental glacier must be given up.

I cannot better indicate the general bearing of facts, as they present themselves to my mind in connection with this subject, than by referring to a paper by Dr. G. M. Dawson on the distribution of drift over the great Canadian plains east of the Rocky Mountains.[2] I am the more inclined to refer to this, because of its recency, and because I have so often repeated similar conclusions as to eastern Canada and the region of the Great Lakes.

The great interior plain of western Canada, between the Laurentian axis on the east and the Rocky Mountains on the west, is seven hundred miles in breadth, and is covered with glacial drift, presenting one of the greatest examples of this deposit in the world. Proceeding eastward from the base of the Rocky Mountains, the surface, at first more than four thousand feet above the sea-level, descends by successive steps to twenty-five hundred feet, and is based on cretaceous and Laramie rocks, covered by bowlder clay and sand, in some places from one hundred to two hundred feet in depth, and filling up preexisting hollows, though itself sometimes piled into ridges. Near the Rocky Mountains the bottom of the drift consists of gravel not glaciated. This extends to about one hundred miles east of the mountains, and must have been swept by water out of their valleys. The bowlder clay resting on this deposit is largely made up of local *débris*, in so far as its paste is concerned. It contains many glaciated bowlders and stones from the Laurentian region to the east, and also smaller pebbles from the Rocky Mountains; so that at the time of its formation there must have been driftage of large stones for seven hundred miles or more from the east, and of smaller stones from a less distance on the west. The former kind of material extends to the base of the mountains, and to a height of more than four thousand feet. One bowlder is mentioned as being forty-two by forty by twenty feet in dimensions. The highest Laurentian bowlders seen were at an elevation of forty-six hundred and sixty feet, on the base of the Rocky Mountains. The bowlder clay, when thick, can be seen to be rudely stratified, and at one place includes beds of laminated clay with compressed peat, similar to the forest beds described by Worthen and Andrews in Illinois, and the so-called interglacial beds described by Hinde on Lake Ontario. The leaf-beds on the Ottawa River, and the drift-trunks found in the bowlder clay of Manitoba, belong to the same category, and indicate that throughout the glacial period there were many forest oases far to the north. In the valleys of the Rocky Mountains opening on these plains there are evidences of large local glaciers now extinct, and similar evidences exist on the Laurentian highlands on the east.

Perhaps the most remarkable feature of the region is that immense series of ridges of drift piled against an escarpment of Laramie and cretaceous rocks, at an elevation of about twenty-five hundred feet, and known as the 'Missouri coteau.' It is in some places thirty miles broad and a hundred and eighty feet in height above the plain at its foot, and extends north and south for a great distance; being, in fact, the northern extension of those great ridges of drift which have been traced south of the Great Lakes, and through Pennsylvania and New Jersey, and which figure on the geological maps as the edge of the continental glacier, — an explanation obviously inappli-

[1] Memoir on glaciers, Geol. soc. Berlin, 1881. Climatic changes, Boston, 1883.

[2] SCIENCE, July 1, 1883.

cable in those western regions where they attain their greatest development. It is plain that in the north it marks the western limit of the deep water of a glacial sea, which at some periods extended much farther west, perhaps with a greater proportionate depression in going westward, and on which heavy ice from the Laurentian districts on the east was wafted south-westward by the arctic currents, while lighter ice from the Rocky Mountains was being borne eastward from these mountains by the prevailing westerly winds. We thus have in the west, on a very wide scale, the same phenomena of varying submergence, cold currents, great ice-floes, and local glaciers producing icebergs, to which I have attributed the bowlder clay and upper bowlder drift of eastern Canada.

A few subsidiary points I may be pardoned for mentioning here. The rival theories of the glacial period are often characterized as those of land glaciation and sea-borne icebergs. But it must be remembered, that those who reject the idea of a continental glacier hold to the existence of local glaciers on the high lands more or less extensive during different portions of the great pleistocene submergence. They also believe in the extension of these glaciers seawards and partly water-borne, in the manner so well explained by Mattieu Williams; in the existence of those vast floes and fields of current- and tide-borne ice whose powers of transport and erosion we now know to be so great; and in a great submergence and re-elevation of the land, bringing all parts of it and all elevations up to five thousand feet successively under the influence of these various agencies, along with those of the ocean-currents. They also hold, that, at the beginning of the glacial submergence, the land was deeply covered by decomposed rock, similar to that which still exists on the hills of the southern states, and which, as Dr. Hunt has shown, would afford not only earthy *débris*, but large quantities of bowlders ready for transportation by ice.

I would also remark, that there has been the greatest possible exaggeration as to the erosive action of land-ice. In 1865, after a visit to the alpine glaciers, I maintained that in these mountains glaciers are relatively protective rather than erosive agencies, and that the detritus which the glacier streams deliver is derived mostly from the atmospherically wasted peaks and cliffs that project above them. Since that time many other observers have maintained like views, and very recently Mr. Davis of Cambridge and Mr. A. Irving have ably treated this subject.[1] Smoothing and striation of rocks are undoubtedly important effects, both of land-glaciers and heavy sea-borne ice; but the levelling and filling agency of these is much greater than the erosive. As a matter of fact, as Newberry, Hunt, Belt, Spencer, and others have shown, the glacial age has dammed up vast numbers of old channels which it has been left for modern streams partially to excavate.

The till, or bowlder clay, has been called a 'ground moraine,' but there are really no alpine moraines at all corresponding to it. On the other hand, it is more or less stratified, often rests on soft materials which glaciers would have swept away, sometimes contains marine shells, or passes into marine clays in its horizontal extension, and invariably in its embedded bowlders and its paste shows an unoxidized condition, which could not have existed if it had been a sub-aerial deposit. When the Canadian till is excavated, and exposed to the air, it assumes a brown color, owing to oxidation of its iron; and many of its stones and bowlders break up and disintegrate under the action of air and frost. These are unequivocal signs of a sub-aqueous deposit. Here and there we find associated with it, and especially near the bottom and at the top, indications of powerful water-action, as if of land-torrents acting at particular elevations of the land, or heavy surf and ice action on coasts; and the attempts to explain these by glacial streams have been far from successful. A singular objection sometimes raised against the sub-aqueous origin of the till is its general want of marine remains, but this is by no means universal; and it is well known that coarse conglomerates of all ages are generally destitute of fossils, except in their pebbles; and it is further to be observed, that the conditions of an ice-laden sea are not those most favorable for the extension of marine life, and that the period of time covered by the glacial age must have been short, compared with that represented by some of the older formations.

This last consideration suggests a question which might afford scope for another address of an hour's duration, — the question how long time has elapsed since the close of the glacial period. Recently the opinion has been gaining ground that the close of the ice age is very recent. Such reasons as the following lead to this conclusion: the amount of atmospheric decay of rocks and of denudation in general, which have occurred since the close of the glacial period, are scarcely appreciable; little erosion of river-valleys or of coast-terraces has occurred. The calculated recession of waterfalls and of production of lake-ridges lead to the same conclusion. So do the recent state of bones and shells in the pleistocene deposits, and the perfectly modern facies of their fossils. On such evidence the cessation of the glacial cold and settlement of our continents at their present levels are events which may have occurred not more than six thousand or seven thousand years ago, though such time estimates are proverbially uncertain in geology. This subject also carries with it the greatest of all geological problems, next to that of the origin of life; namely, the origin and early history of man. Such questions cannot be discussed in the closing sentences of an hour's address. I shall only draw from them one practical inference. Since the comparatively short post-glacial and recent periods apparently include the whole of human history, we are but new-comers on the earth, and therefore have had little opportunity to solve the great problems which it presents to us. But this is not all. Geology as a science scarcely dates from a century ago.

[1] *Proc. Bost. soc. nat. hist.*, xxii. *Journ. geol. soc. Lond.*, Feb., 1883.

We have reason for surprise in these circumstances, that it has learned so much, but for equal surprise that so many persons appear to think it a complete and full-grown science, and that it is entitled to speak with confidence on all the great mysteries of the earth that have been hidden from the generations before us. Such being the newness of man and of his science of the earth, it is not too much to say that humility, hard work in collecting facts, and abstinence from hasty generalization, should characterize geologists, at least for a few generations to come.

In conclusion, science is light, and light is good; but it must be carried high, else it will fail to enlighten the world. Let us strive to raise it high enough to shine over every obstruction which casts any shadow on the true interests of humanity. Above all, let us hold up the light, and not stand in it ourselves.

LETTERS TO THE EDITOR.

*** *Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Kalmias and rhododendrons.

June 16 of the present summer I chanced to be floating down Crossweeksung Creek in my canoe; and, at a bend in the stream, found myself at the foot of a steep bluff some seventy feet high, which was densely covered with a luxuriant growth of kalmias and rhododendrons in full bloom. The former were laden with magnificent clusters of white, waxy flowers; and the more gorgeous pink rhododendron-blossoms were scattered through them. It was the most beautiful floral display I had ever seen.

On my return home, I turned to the description by Kalm of the smaller of these shrubs, to which Linné gave the generic name it now bears in honor of its discoverer. Kalm writes, "Linnaeus, conformable to the peculiar friendship and goodness which he has always honored me with, has been pleased to call this tree Kalmia." He further says, "The spoon-tree, which never grows to a great height, we saw this day in several places. The Swedes here have called it thus, because the Indians, who formerly lived in these provinces, used to make their spoons and trowels of the wood of this tree. In my cabinet of curiosities I have a spoon made of this wood by an Indian." Again he says, "About the month of May they begin to flower in these parts (central New Jersey), and then their beauty rivals that of most of the known trees in nature. The flowers are innumerable, and sit in great bunches," etc.

Kalm was visiting in New Jersey when he wrote the above; and it may be that where he was at the time (Swedesboro, Gloucester county), the rhododendron is not found. At all events, he nowhere mentions this shrub, which is here known as 'mountain laurel' to distinguish it from the true kalmia. In calling the latter the 'spoon-tree,' has he confounded the two? Certainly his remarks on the character of the wood, and the use to which it was formerly put by the Indians, lead to that conclusion. At present, it would be difficult to find a sufficiently large growth of kalmia to enable an Indian to whittle from it a spoon or trowel of respectable size. From rhododendron-stocks, implements of considerable size can be made; and Professor Kalm's description of kalmia wood is equally applicable to it. He describes it as "very hard, may be made very smooth, and does not easily crack or burst."

In Britton's Flora of New Jersey, Kalmia latifolia is called 'spoon-wood,' which name, I suppose, is derived from the remarks made by Kalm, as above quoted. I suggest that it is a misnomer, and that the remarks on the uses of the wood made by the distinguished Swedish naturalist refer really to the rhododendron.

Considering that Kalm was so careful an observer, was particularly interested in botany, and further, not only enjoyed the friendship of Bartram, but frequently visited him, in whose celebrated garden was a rhododendron-grove, it is strange that no mention is made, in his 'Travels in North America,' of the larger 'laurel,' so called; yet such appears to be the case.

This is an unimportant matter perhaps, but, if I am right, should not go uncorrected.

Charles C. Abbott, M.D.

Trick of the English sparrow.

A curious freak of the imported sparrow recently came to my notice at Basin Harbor, on Lake Champlain, in Vermont.

The eaves-swallows had attached their mud 'retorts,' as usual, in line under the eaves of the farmer's barn, anticipating, no doubt, a successful and happy house-keeping, notwithstanding a colony of feathered foreigners had encamped about the premises.

At sight of these 'bottle-nosed' dwellings, now arriving at completion, it occurred to the little tramps that these were exactly the thing they wanted; but, as the apartments were not to let, a battle ensued, which resulted in the rout of Lunifrons. The sparrows then took possession of the mud-houses, and furnished them to their own taste. But some of the 'masons' made a successful resistance, and still held the castle; so that often a swallow-family had their arch enemy at next door.

Thus in more ways than one does the impudent little urchin, which has come to us from over the sea, merit the name of *parasite*. Now that the bird has become not only a general nuisance, but a sore annoyance to our native and useful birds, it is no wonder if the cry goes up all over the land, '*The sparrow must be blotted out!*'

F. H. Herrick.

Achenial hairs of Senecio.

In a paper read before the American association for the advancement of science at Montreal, Professor Macloskie referred to the achenial hairs of some of the Compositae. The paper was afterward published in the *American naturalist* for January, 1883; and here we find a figure showing the tubes issuing from the hairs of Senecio. A beautiful experiment showing these tubes, or rather threads, can be made with the achenes of S. Douglasii. Scraping a few of the hairs from an achene, and placing them on a slide under the microscope with a two-thirds objective, and applying a drop of water to the slide, the threads are seen to uncoil. As soon as the water touches the hairs, the tips seem to burst, and allow the threads to emerge, rapidly twisting round and round in a very snake like manner. The experiment is a most satisfactory one, and can be readily made. These threads were noticed long ago, as Lindley (Veg. king., p. 704–705) speaks of Decaisne having seen them. Lindley says in regard to them, "On placing one of these papillae in water, it immediately separates into two lips, and these emit mucilaginous tubes, which issue forth like wires, spirally unrolling themselves, and finally much exceed the papillae from which they proceed. These tubes are apparently formed by a very considerable number of threads placed one upon the other

in the manner of a skein of thread." I do not know of any explanation of the use of these threads. Can any of your readers suggest a purpose for them?

JOS. F. JAMES.

Cincinnati, O., Aug. 2, 1883.

Seeds of Lepidium.

I regret to observe, by your issue of July 27, that my employment of the expression 'mucilaginous threads' as to the seeds of Lepidium has led your reviewer to understand that I referred to something like the seed-fibres of Collomia. Spiral fibres embedded in mucilage are found on the seeds of Collomia; radiating processes consisting of mucilage, each tipped by a facet of cuticle, are emitted by the seeds of Lepidium virginicum. This is shown on the application of water with staining-fluid to ripe seeds. Other species of Lepidium (including L. ruderale) show the same phenomenon, though the experiment may fail with immature seeds or old herbarium specimens.

G. MACLOSKIE.

Princeton, N.J., Aug. 3, 1883.

["The exotest may bear long hairs (cotton) or spiral threads. . . . In Lepidium (pepper-grass), on being moistened, it darts out mucilaginous threads." It certainly may be gathered from this that the 'spiral threads' and the 'mucilaginous threads' are not the very same. But the darting-out of mucilaginous threads so well describes what one sees in Collomia-seeds and the like, and so poorly answers to what takes place in those of Lepidium, that the reviewer supposed there might be some mixing up of cases. But he simply asked whether the author was sure of the threads in Lepidium. We find nothing to which the name of 'mucilaginous threads' can with any exactness be applied; nor do we think that the term now used of 'radiating processes,' though not widely amiss, gives a clear idea of the case, which we should describe thus: —

A superficial pellicle of the seed-coat of Lepidium consists of a single and continuous layer of cells, the thick walls of which are at maturity converted into mucilage, or into an isomer of cellulose, which swells up into mucilage 'upon the application of water.' But the water acts so promptly in forming the *limbus* around the seed or its section, that we fail in that way to get an intelligible view of the structure and the nature of the process. To do this, however, we have only to soak thin sections of the seed in strong alcohol, examine in them the unaltered mucilage-cells, and then add a little water by degrees. The cells then swell up slowly, push outward radially (for mutual pressure prevents lateral expansion at the beginning), become wedge-shaped or pear-shaped as they farther protrude, and at length form the well-known mucilaginous *limbus*. Dr. Macloskie will be interested in repeating this experiment, and will accept our apology for partially misunderstanding him.]

KONKOLY'S ASTRONOMICAL INSTRUMENTS.

Praktische anleitung zur anstellung astronomischen beobachtungen, mit besonderen rücksicht auf die astrophysik, nebst einer modernen instrumentenkunde. Von NICOLAUS VON KONKOLY. Braunschweig, *Vieweg*, 1883. 912 p., 345 illustr. 8°.

THIS is an important but at the same time a disappointing work. It contains the description and representation of nearly all the principal modern astronomical instruments, and presents such a comprehensive summary as can be found in no other existing book. The numerous illustrations, largely derived from the business catalogues of leading instrument-makers, are generally excellent, and the mechanical execution and press-work are admirable. Undoubtedly the book is one which must have a place in every astronomical library.

At the same time, the work is far from exhaustive, omitting all mention of many of the latest and most useful improvements; and it is not always accurate in its description of those it does notice. Nor does it deal in any thorough or satisfactory manner with the theory of the instruments described. It is so full and so good, that it is a great pity that it is not still better and still more complete, as it easily might have been.

The first chapter, on time-keepers (*uhren*), describes, among clock-escapements, only the old Graham dead-beat and a duplex of Jürgensen's. There is no notice of Airy's detached escapement, now in use at Greenwich, nor of any of the numerous and excellent gravity-escapements now so common in England and this country. The account of electric make and break circuit apparatus is for this reason unsatisfactory, since only escapements of the detached class admit of a simple break-circuit which does not affect the pendulum. The author treats the subject rather extensively, describing no less than twelve different forms of contact apparatus, some of them very elaborate and complicated. The antiquated contrivances of Locke and Mitchell are described as if they continued to be in use.

The second chapter, a short one, deals with the different forms of levels and level-testers, and appears to be in all respects satisfactory.

The third chapter treats of instruments for the determination of time. Under this head are included not only transits and transit-circles, but all forms of theodolites, sextants, passage-prisms, etc. There is also a certain amount of information respecting the graduation of circles and the methods of testing their accuracy, i.e., the optical and mechanical arrangements; the mathematical theory remaining untouched.

The next chapter, the fourth, is by far the most extensive and full of any, occupying two hundred and forty-six pages. It treats of equatorials and their mounting, and describes and illustrates nearly all the important modern telescopes. For the most part, it is well done, especially the portion relating to driving-clocks,

It is evident, however, that the author does not fully grasp all the principles involved in these machines, or he would hardly have spoken so disparagingly of the 'spring-governor' of Bond, which is unquestionably, when properly adjusted, one of the most perfect of all. In so full a treatment of the subject, one would naturally expect to find some notice of the ingenious arrangement by which the clock-work of the Dun Echt equatorial is brought under the electric control of the standard time-piece; but it is missing, though Grubb's less perfect apparatus for the same purpose is fully described.

The fifth chapter, dealing with micrometers, calls for no special notice, beyond the remark that it strikes one as a curious classification which treats of *chronographs* in this connection.

The sixth chapter is a short one, describing the different forms of helioscopes and solar eye-pieces, and the most convenient arrangements for making drawings of sun-spots and determining their position.

The seventh chapter is intended to be a full and elaborate description of the different forms of astronomical spectroscopes, with their accessories. It does describe and figure a great many; but there are several mistakes (as, for instance, on p. 656, where the temporary device which Professor Young employed in observing the eclipse of 1869 is said to have been used with a heliostat, and is spoken of as if it were now used at Princeton), and there is the capital omission of failing even to mention the use of diffraction-gratings in spectroscopic work. It strikes one as very surprising that the author should not have learned that for solar observations the grating has almost entirely supplanted the prism in many if not most observatories. The remarkable apparatus of Thollon is alluded to, but not described with any fulness.

The remaining chapters of the book treat of apparatus for celestial photography, photometry, and the measure of solar radiation.

Similar remarks apply to these as to the preceding. There are many excellent descriptions and illustrations, many important omissions, and a few mistakes. We call special attention to the fine representation of the most ingenious mounting — devised by Hansen, and constructed by Repsold — for the photoheliographs employed by the German transit of Venus parties, — a contrivance which we have never seen described elsewhere. But in the chapter on photography, neither the name of H. Draper nor of Common appears; and Rutherford's photographs of the spectrum are said (on p. 827) to have been made with an apparatus he never even saw, the instrument figured being a spectroscope which was used at Dartmouth college in attempting to photograph the solar prominences, while the description given is incorrect in several particulars. In the chapter on the measurement of radiation the apparatus of Pouillet and Secchi appears, but nothing later, — none of the instruments of Violle or Crova, and, of course, not the bolometer of Langley. The chapter on photometers is much better brought up to date.

On the whole, the book is rather a provoking one. There is a great deal in it of real value, collected from various more or less inaccessible sources, and very neatly presented; but the *lacunae* are serious, and a few detected mistakes leave a sense of insecurity as to accuracy in other details.

BURNHAM'S LIMESTONES AND MARBLES.

History and uses of limestones and marbles. With forty-eight chromolithographs. By S. M. BURNHAM. Boston, *S. E. Casino & Co.*, 1883. 15 + 392 p. 8°.

THE separate crystals of our rocks, when they lend themselves to decoration in the form of gems, afford a capital opportunity for the book-maker. Superstition, tradition, a host of human activities, have gathered about them, that, in the hands of writers of skill, have been worked into very readable books. But, when the author of 'Limestones and marbles' tries to take something of the same book-maker's way with the coarser though still beautiful marbles, he leaves the field of thoroughly humanized things, and finds himself in a dreary sea of unrelated facts. A writer thoroughly conversant with the architectural history of building and ornamental stones could probably give us a book which would, from its connection with the most economic of the fine arts, be very readable. A skilled lithologist who would furnish us a careful discussion of the nature of those changes which give beauty, strength, and endurance to rocks, would thereby furnish us with a needed essay; but in this book we have no trace of these capacities, but only the ordinary patience of the devoted compiler.

As a piece of unwearied compilation, unenlivened with any higher quality, this is a very remarkable book. In the list of limestones of the United States we have evidence of a most universal but most uncritical ransacking

of authorities; for the element of personal knowledge is entirely wanting. Nor has the compilation the value it might have had if authorities had been quoted. Although the book is apparently by a New-Englander, he omits the limestones of Smithfield, R.I., and the serpentines of Lynnfield, Mass., — both interesting, though, as yet, little-used stones. Any personal knowledge of the subject would have supplied a host of such facts, which are not to be found in books, though well known to geologists. The same absence of personal knowledge leads to such misleading statements as that the fossils around Prague are identical with those of the same age in Scandinavia, Russia, Great Britain, and North America. While the book is padded with thirty-eight pages on classification of fossils, nothing is given to the arts of quarrying or of dressing stones, — most important and most relevant matters.

The chromolithographic plates are fairly well done: they fail to give the peculiar effect of depth or translucency, which is beyond this art, but which is the greatest charm of the finest decorative stones.

The style is not altogether bad, though it is frequently inverted; and the author often gets into the subject very much as John Phoenix 'backed the transit' into the plane of the meridian. Now and then it is strikingly epigrammatic, as in the following phrase: 'One of the caprices of nature is to anticipate the works of art.'

It is a pity that so much faithful labor should have been given to this work. The printing of the book, and the index, are very satisfactory. Despite its defects, the book will have a certain value to those interested in the subject; for, as a compilation, it is, in its way, remarkable.

A PRIMER OF VISIBLE SPEECH.

Visible-speech reader for the nursery and primary school. By ALEX. MELVILLE BELL, F.E I S., etc. Cambridge, *King*, 1883. 4 + 52 p. 16°.

THE science of phonetics made, perhaps, its greatest advance through Bell's Visible speech, though it has by no means remained stationary since that book appeared. It is this system which this primer seeks to bring into practical use in teaching, and its alphabet is a great improvement over that which we now use. It cannot be said, however, that the phonetic analysis on which it is based has received in all respects the approval of phoneticians. With some changes, the vowel system has now won wide acceptance, but the analysis of consonants has met with serious objections; for instance, for such sounds as *f*, *th*, *s*, *sh*, in English. A discussion of the system itself would necessitate reference to recent work on phonetics, especially to Sweet's paper on Sound notation in the Transactions of the philological society for 1880–81, and to Sievers's Grundzüge der phonetik, and such a discussion would hardly be in place here. One may wish, however, that some of Sweet's changes of the Visible-speech alphabet could have been adopted. Still, the imperfections of the system might never attract a child's notice, and he would probably accept unquestioningly the signs given for *f* and *th*, without understanding why they were made to resemble the sign for *l*. For the scientific study of living languages, and of the phenomena of linguistic change, some such phonetic system as Visible speech, we may hope, will be agreed upon, at least provisionally, whether it is found of practical value in teaching children to read or not. The test of practice must show whether this ingenious alphabet will do better than other phonetic primers the work of teaching a child to read ordinary printed books. The primer is divided into three parts, — first, pictured words, containing pictures of a few common objects, with their names and some phrases; next, sentences in rhythmical form; and lastly, a vocabulary of common words arranged according to the initial sound, beginning with labial consonants, and ending with vowels. All this is printed only in Visible-speech letters. These three parts are preceded by some directions to the teacher; and at the end a key is added for the teacher's use, containing the usual forms in Roman type of all the words in the primer. Exclusive of the key, the whole contains thirty-five pages. At the beginning of the key are given a few 'notes,' which speak of the syllabic *l* and *n*, as in *castle*, *listen*, and of the glides, that is, the vowel vanishes, or final diphthongal elements in such words as *hear* (the sound represented by *r*), *day*, *go*. It must surprise an American student of phonetics to see that American pronunciation is credited by Mr. Bell with pure long vowels in the last two of these words, instead of with diphthongs, especially if his own experience and observation with foreign languages have shown him how hard it is for most Americans to learn the pure long sounds of *e* and *o* as pronounced on the continent of Europe. Possibly the American vanishing vowel in these cases is less prominent than in England, and it may be that some Americans do pronounce

simple long vowels in such cases. In this primer these two glides are not used with ā and ō. To call the *r* glide, as in *hear*, a very soft *r* is misleading, as most of us in the eastern United States pronounce absolutely no *r* at all in such words.[1] Here, too, what is said of American pronunciation is inexact; for surely we all have an *r* glide in words like *hearing*, while an English reader of Mr. Bell's words would suppose that Americans pronounce *hear* as he does, but *hearing* like *he-ring*. The American rule for the *r* glide may be thus stated for some, perhaps most of us: when the *r* glide is present at the end of a word, it is retained before any ending of derivation or inflection, the consonant *r* being pronounced in addition after the glide if the ending begins with a pronounced vowel. Thus the glide is heard in *boor*, *boorish*, *beer*, *beery*, *soar*, *soaring*, *store*, *storing*, *stored;* but there is no *r* glide in *Mary*, *story*, *fury*. Cases like these last seem to have been excluded from the book.

[1] See Whitney, The elements of English pronunciation, in his Oriental and linguistic studies, second series.

WEEKLY SUMMARY OF THE PROGRESS OF SCIENCE.

CHEMISTRY.

(General, physical, and inorganic.)

New explosive. — S. H. Hinde proposes a new explosive mixture composed of 64 parts of nitro-glycerine, 12 ammonium citrate, 0.25 ethyl palmitate, 0.25 calcium carbonate, 23 coal, 0.50 sodium carbonate. — (*Chem. techn. repert.*, 1883, 153.) C. E. M. [196

Compressed cartridges. — H. Güttler makes cartridges of compressed blasting-powder, which are bound together by dextrine. For this purpose he uses a hard burned charcoal (brown-red), which he claims has the formula $C_8H_4O_2$. The mixture of charcoal, sulphur, and nitre are incorporated with the solution of dextrine, corned in grains of one to two millimetres; and after drying they are pressed into perforated cylinders. These cylinders are then dried and shellacked. The reaction due to explosion is represented, when India nitre is used, by $C_8H_4O_2+8\,KNO_3+4\,S = 8\,CO_2+2\,H_2O+8\,N+2\,K_2SO_4+2\,K_2S$. — (*Chem. techn. repert.*, 1883, 154.) C. E. M. [197

Fulminating compound. — B. G. and F. L. Benedict have invented a mixture for use in primers, in place of fulminating mercury, consisting of 2 parts amorphous phosphorus, 8 of minium, and 2 of potassium chlorate. The oxides of mercury or manganese may be used in place of the minium. — (*Chem. techn. repert.*, 1883, 153.) C. E. M. [198

AGRICULTURE.

Soluble and insoluble phosphates. — In experiments on potatoes, Swanwick and Prevost obtained a larger yield on plots manured with superphosphate than on those manured with the same phosphate simply ground. A slight increase in the percentage of starch was observed in the potatoes manured with superphosphate. — (*Bied. centr.-blatt.*, xii. 250; *Trans. highl. agric. soc.*, 1882.) H. P. A. [199

Value of artificial butter. — There are, according to Ad. Mayer, three principal points to be regarded in judging of the worth of an article of diet; viz., harmlessness, taste, and physiological utility. That artificial butter is harmful can hardly be seriously claimed; while, as regards its taste, the very magnitude of the industry shows that the imitation is very successful. The physiological utility of artificial butter depends essentially on its digestibility; and on this point Mayer has experimented, using as subjects a man, and a boy nine years old. But slight differences were observed between natural and artificial butter; but the former was digested a trifle better. When the artificial butter was used in preparing potatoes, it proved to be almost uneatable; and the author suggests that this fact may prove of use in detecting the presence of the former. — (*Landw. vers.-stat.*, xxix. 215.) H. P. A. [200

Butt and tip kernels of corn. — The vegetation of the butt, central, and tip kernels of corn in the field has corroborated the results already published as gained in the greenhouse. The figures of vegetations stand as below: —

Planted.	Butt kernels.		Central kernels.		Tip kernels.	
	June 1.	June 4.	June 1.	June 4.	June 1.	June 4.
1 A 1, May 16	446	533	551	581	564	600
1 A 2, "	478	534	515	564	564	583
1 A 3, "	497	558	490	570	500	549
1 A 4, "	428	496	463	560	519	587
1 A 5, "	362	467	456	526	428	526
Total vegetated	2211	2588	2485	2801	2575	2845
Total planted	3420	3420	3420	3420	3420	3420
Per cent vegetated . .	64	75	72	82	75	83

— (*N.Y. agric. exp. stat., bull.* xlvii.) H. P. A. [201

Chemistry of asparagin. — B. Schulze finds that asparagin is not decomposed to any notable extent by heating with water, even under a pressure of three to four atmospheres, and in the presence of acid plant-juices. Consequently, when fodders containing asparagin, of which there are many, are cooked, this substance is unaltered; and, since its nutritive value has been established, the knowledge of this fact is of some importance. When heated with alkalies, asparagin yields asparaginic acid and ammonia, while a portion of the acid is further acted on, and malic acid is formed. — (*Landw. vers.-stat.*, xxix. 233.) H. P. A. [202

METEOROLOGY.

Observations on Ben Nevis. — A permanent observatory is to be established at the summit of this mountain by the Scottish meteorological society. A road to the summit has been begun: the building will be erected this summer, and it is expected that regular observations will be made after Nov. 1. The records will be kept hourly, not only at the summit, 4,406 feet above sea-level, but also at Fort William, which is situated twenty-eight feet above the sea, and at the base of the mountain. Since June 1, 1881, simultaneous observations at these points have been made at frequent intervals of the day, in the summer-time, by Mr. and Mrs. C. L. Wragge, the former of whom made the ascent every day until the storms of October rendered this impossible. The results obtained have been discussed by Mr. Buchan sufficiently to warrant the permanent establishment of the observatory. — W. U. [203

The origin of lightning. — In explaining satisfactorily the phenomenon of lightning, a difficulty is encountered in accounting for the enormous electric tensions which are necessary to explain the great length of the spark often observed. The theory is advanced by A. Fick, that the high tensions are produced by the sudden concentration of electricity already existing in a free state. This concentration is caused by the formation of large drops of rain from the small vesicles of moisture existing in the clouds, by which the surface upon which the electricity exists is greatly diminished. The sudden formation of drops of water from the mass of aqueous vapor may be due to the advance of cold-air currents. The author endeavors to answer two objections which may be urged against his theory: 1. That in every rain-storm lightning ought to be seen; 2. That it ought to rain whenever it lightens. To the first objection he replies, that the drops may be formed gradually, and not suddenly, in which case the tensions would be dissipated gradually; and, to the second, that drops are always formed in connection with lightning, but that in falling to the earth they sometimes encounter a layer of dry air, and are absorbed in their passage. — (*Naturforscher*, June 23.) W. U. [204

GEOGRAPHY.

(Arctic.)

News from Bering Sea. — News to July 8 has been received from the North Pacific whaling-fleet. The promise of a late spring had been fulfilled to date. Large quantities of drift-ice were afloat in Bering Sea some distance south of Bering Strait as late as the end of June. The whalers had taken but few whales, — only nine for the whole fleet. St. Lawrence Bay did not open until July 1. The Leo, bound for Point Barrow to relieve the party at the U. S. international polar station, had arrived at Plover Bay July 5. During the last few days of June strong southerly winds prevailed, driving the ice northward, so that at least one of the steam-whalers was able to reach ten leagues north of Cape Lisburne. The Corwin had not arrived. The bark Mary and Susan had been nipped, and was leaking badly; and the steam-whaler Balaena had returned to Plover Bay with the loss of her propeller-blades. Most of the fleet met south of St. Paul Island, in latitude 57° N., in April, and were fast in the ice from forty to eighty days, encountering very heavy ice and severe cold. The whales in their northward migration passed Cape Chaplin about July 9. The bark Hunter had been injured by a serious fire in the forecastle. A small number of walrus had been taken in default of larger game. Notwithstanding the unfavorable spring, a few weeks suitable weather may change the conditions sufficiently to enable the fleet to make a fair season's catch; but it must be confessed that the prospect of this, as well as for the Leo's reaching Point Barrow, and securing the desired observations there, are not encouraging. — W. H. D. [205

(Africa.)

Revoil's journey to Somali-land. — M. G. Revoil, recently intrusted with the direction of an expedition to Somali-land by the French ministry of public instruction, left Zanzibar about the first of May. During detentions at Aden and Zanzibar, collections of natural history and ethnology were obtained, and the members of the party instructed in the methods of work. Friendly relations were established with several chiefs of the Somali coast, who were on an annual visit to Zanzibar, and recommendations to various tributary chieftains obtained from the sultan. M. Revoil intended to enter the country with Arab guides at Mogadoxo. and to ascend the Wabbi River to Geledi, whence, after a short stay, he would proceed to Gananeh on the Juba River, which he would endeavor to map, while obtaining collections of all kinds. After this the Juba would be ascended to the region of the Ugadines toward the west, or he would enter the Galla country toward Kaffa and Shoa, where it is thought the friendly relations of the French with King Menelik would insure him a favorable reception. It is expected that the journey will terminate by traversing the country to Harrar, and thence to Zeila on the Gulf of Aden. — (*Comptes rendus soc. géogr.*, no. 11.) W. H. D. [206

ZOÖLOGY.

Mollusks.

Existence of a shell in Notarchus. — Vayssière has demonstrated the existence of a minute internal spiral shell in Notarchus. Taken into consideration with a similar discovery by Krohn in Gasteropteron, the author thinks it very probable that both are persistent embryonic shells (in Notarchus it is about one-fiftieth as long as the animal itself), and that an analogous appendage will be found eventually in most tectibranchs, which, up to the present time, have been considered shell-less. — (*Journ. de conchyl.*, xxii. 4.) W. H. D. [207

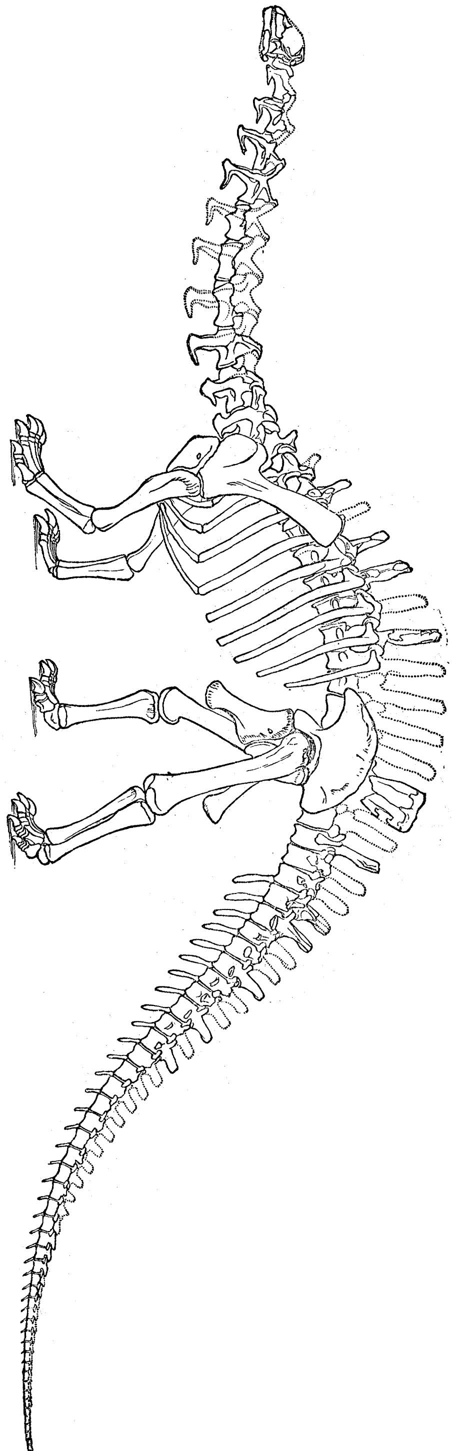
New abyssal mollusks. — Fischer describes a number of new species from the deep-sea dredgings of the Travailleur in 1882. They belong to the genera Dentalium, Mitra, Sipho, Pseudomurex, and Belomitra. The latter is a new genus resembling Bela, but with numerous small plications on the columella.

One species, Mitra cryptodon, comes from a depth of 1,900 metres in the Atlantic, — probably the greatest depth recorded for any species of that genus up to the present time. — (*Journn. de conchyl.*, xxii. 4.) W. H. D. [208

VERTEBRATES.

Reptiles.

Restoration of Brontosaurus. — In the continuation of his papers on Sauropoda, Marsh gives the accompanying restoration of Brontosaurus almost entirely from a single individual about fifty feet long. "The head was remarkably small; the neck was long, and, considering its proportions, flexible, and was the lightest portion of the vertebral column; the body was quite short, and the abdominal cavity of moderate size; the legs and feet were massive, and the bones all solid; the feet were plantigrade, and each footprint must have been about a square yard in extent; the tail was large, and nearly all the bones solid." Special attention is drawn to the head, which is "smaller in proportion to the body than in any vertebrate hitherto known," the entire skull weighing and measuring less than the fourth or fifth cervical vertebra. The animal is estimated to have weighed more than twenty tons, was more or less amphibious, probably fed on aquatic plants, and was doubtless a 'stupid, slow-moving reptile,' wholly wanting any offensive or defensive weapons. — (*Amer. journ. sc.*, Aug.) [209



RESTORATION OF BRONTOSAURUS.

Mammals.

Influence of pressure on heart-beat. — Many observers have noticed that the mammalian heart, after the death of the animal, will, under certain conditions, continue to beat spontaneously for some hours, especially if artificial inflation of the lungs is kept up. Ewald and Kobert have made some observations on this subject, inflating the heart directly with air, and find that hearts which have ceased to beat spontaneously, or after the application of mechanical stimuli, will again give contractions when the pressure within their cavities is raised. They come to the conclusion that one of the conditions which the blood must fulfil, in order to maintain the heart in activity, is, that it must exert a certain pressure on the heart-walls. — (*Pflüger's archiv*, xxxi. 187.) W. H. H. [210

Epiphyses on the centra of the vertebrae of the manatee. — M. Albrecht describes these rudimentary epiphyses at length. He believes that the presence of crests and furrows upon the intervertebral faces is a sure indication of epiphyses; but he goes further, and describes these processes. They are 'partially ossified in a peripheral zone, particularly in the dorsal region.' He also forms the hypothesis that the epiphyses are the remnants of more perfect ones, basing it upon the fact of the presence of the ridges and grooves upon the faces of the centra. — (*Bull. mus. hist. nat. Belg.*, ii. 1883, 38.) F. W. T. [211

ANTHROPOLOGY.

The skulls of assassins. — A short time since, attention was called to the investigations made upon criminals and delinquents, with a view to study the

early stages of humanity. The discussion is kept up by the French society, and most elaborate measurements are reported. M. Dally is not quite satisfied with the methods, however, and makes the following remarks. It is very wrong to confound things different *inter se* under one abstract term, and to study them as a natural group. Assassins, murderers, criminals, and even the assassinated, constitute juridical categories; but surely they are not philosophic. Highwaymen, ravishers, the jealous, monomaniacs, avengers, nihilists, etc., may be assassins; yet they have nothing in common, except that their actions lead to the same result. The organic conditions which lead to murder are quite different in each case. Again: every one knows that nothing is more rare than a perfectly symmetrical skull. Before establishing the proportions of anomalous crania among criminals, it is necessary to fix the standard among the virtuous. In fact, all men who have heavy lower jaws are not necessarily assassins; nor can we assume that all crime is evidence of atavism, and argue, hence, that in the anatomy of murderers we have the portraits of our prehistoric ancestors. — (*Bull. soc. anthrop. Paris*, v. 778.) J. W. P. [212

Easter Island. — Commander Bouverie F. Clark, in June last, visited the Easter Island, landing at the village of Malaveri, where the vessel was boarded by Mr. Alexander Salmon, agent of the Maison Brander of Tahiti, who purchased the property of the missionaries four years ago. The latter then left for the Gambier Archipelago, taking three hundred natives with them. The natives now number a hundred and fifty, and are decreasing. About five hundred were shipped to Tahiti eight years ago, to work on the plantations of the Maison Brander. Among the remaining people are no traces of the missionary work. They are divided into several small clans; and their chief quarrels are about the first eggs of the 'wide-awake' every year from Needle rock. The myth or tradition of their arrival is given by Commander Clark, who also speaks hopefully of the fertility of the island, as well as its value as a provision station. — (*Proc. roy. geogr. soc.*, v. 40.) J. W. P. [213

INTELLIGENCE FROM AMERICAN SCIENTIFIC STATIONS.

PUBLIC AND PRIVATE INSTITUTIONS.

University of Michigan.

Central laboratory for microscopy and general histology. — Instruction is given in this laboratory in the following subjects. 1. Microscopical technics, or the science and art of microscopy, comprising, (*a*) the theory and construction of the instrument and its various accessories; (*b*) the methods of determining magnifications; (*c*) the methods of microscopic drawing, microscopic photography, and microscopic projections; (*d*) the preparation of objects of various classes. 2. Human histology. 3. Comparative histology. 4. Vegetable histology. 5. Dental histology. 6. Pathological anatomy. 7. Completion of microscopic study in such other subjects as may be desired by professors in charge.

The following is the plan pursued in the principal divisions: —

Normal human histology. — This course consists of thirty lectures in the amphitheatre on the use of the microscope and on histology. In laboratory work the student is taught the manipulation of the instrument, use of accessories, etc. Then follows the study of such subjects as blood, epithelium, bone, tooth, cartilage, elastic tissue, muscle, kidney, stomach, liver, intestine, brain, spinal cord, and various miscellaneous subjects, as the oesophagus, tongue, skin, etc. The students are given instruction in mounting, so that each specimen is preserved as it is studied. The average number of mounts per student is about twenty. Each student is required to have at least twelve mounts, and some ambitious ones mount as high as fifty or sixty. Over six thousand mounts are carried away each year by students in this department. The object of the course is, first, to make the student better acquainted with the structure of tissues, and, second, that he may become familiar enough with the microscope and its manipulations to work to advantage without the aid of an instructor.

Vegetable histology. — The first course consists of work in structural botany for a term of twenty weeks. Special attention is given to the correct representation of microscopic objects on paper. Sixty accurate drawings of the various structures examined during the course are required of each student, the specimens being prepared by the students themselves. Vegetable protoplasm is studied with the special view of ascertaining the effects of the various reagents employed in general laboratory work. Then follow lessons on the vegetable cells, diatoms, and other miscellaneous subjects.

Course two in vegetable histology consists of work in pharmaceutical botany, three forenoons of laboratory work each week for twenty weeks. At the close of the course each student chooses a particular drug, studies it thoroughly, and presents the results of his labors in the form of a thesis.

Advanced normal and pathological histology. — Any student who has completed the primary course in the histological laboratory, or who has performed an equivalent amount of work in some other institution, can enter the class for advanced work. The first work here is in testing objectives with test-plates and diatoms, and in becoming more familiar with a few useful accessories. The art of injecting is then taken up, and the frog and cat are experimented upon, as well as individual organs from larger animals. Each student then chooses some particular organ or tissue, and prepares it in as many ways as possible for study. He thus becomes

familiar with the various methods of hardening, cutting, and staining. Pathological structures are now carefully studied. This includes the study of inflammation and its results, the study of diseased organs and tissues, and of the non-inflammatory new formations.

Embryology.—A study of the development of the chick, including microscopic sections of the same.

Urinalysis.—A course of six weeks in the chemical analysis of the urine, including the use of the microscope in determining the character of the various deposits and crystals.

NOTES AND NEWS.

Dr. H. Newell Martin, professor of biology in Johns Hopkins university, has been appointed Croonian lecturer of the Royal society of London for the current year. The Croonian lecture was founded by Lady Sadlier, in fulfilment of a plan of her former husband, Dr. Croone, one of the founders and the first registrar of the Royal society. By her will, made in 1701, she devised "one-fifth of the clear rent of the King's-Head Tavern, in or near Old Fish Street, London, at the corner of Lambeth Hill, to be vested in the Royal society, for the support of a lecture and illustrative experiment on local motion." For many years past there has been no formal delivery of the lecture. The council of the Royal society select from the papers presented to them during the preceding twelve months that one dealing with animal motion which they think most noteworthy, and publish it as the Croonian lecture, sending to the author the sum derived from Lady Sadlier's bequest. The amount of money is trivial, but the appointment as Croonian lecturer is a highly prized distinction. The paper by Professor Martin, which is to be printed as the Croonian lecture for 1883, is on the Effect of changes of temperature on the beat of the heart. It is interesting to note that the first Croonian lecture, delivered by Dr. Stuart in 1738, was on the Motion of the heart.

—*Nature* of Aug. 2 prints the following telegram from the Swedish party which wintered at Spitzbergen, and was last heard from in October. "Cape Thordsen, July 4, 1883. This message will be forwarded to-morrow to Capt. Startschin, with the boat fetching our first mail this year. The wintering of the expedition has in every respect been attended with success, particularly as the scientific researches have throughout been carried on exactly in accordance with the regulations formulated by the International polar commission. Hydrographical and magnetic studies have also been pursued on the ice in the Ice Fjord, as well as parallax measurements of clouds, and observations as to the temperature of the air, the snow, and the earth. The winter has, on the whole, been mild; the greatest cold occurring on Jan. 2, when the thermometer registered 35.5° C. below freezing-point. Storms have been few. Since September last the following buildings have been erected: a hut on a mountain at an elevation of 270 metres, containing the anemometer and the wind-fan, which were read by a self-registering electrical apparatus; two astronomical observatories; another magnetic hut; a bath-house, a forge, and a wood storehouse. The dwelling-house and working-room have also been enlarged. The following game was shot during the winter: 61 ptarmigans, 9 reindeer, 18 wild geese, 20 foxes, and some wild fowl. With continuous labor, plenty of food and drink, and frequent baths, the members of the expedition have throughout enjoyed excellent health. Descriptions of the nature of our labor and life here during the wintering will follow."

—The new biological laboratory of the Johns Hopkins university, which will be opened next September, has been especially constructed with reference to providing opportunity for advanced work in experimental physiology. It contains two large rooms for general advanced work in animal physiology, in addition to others specially designed for work with the spectroscope, with the myograph, for electro-physiological researches, and for physiological chemistry. It also contains a special room constructed for advanced histological work, and well supplied with apparatus and reagents, a room for micro-photography, and rooms for advanced work in animal morphology.

Prof. C. H. F. Peters of Clinton, N.Y., announces to Harvard college observatory the discovery of a new planet by him on the night of Aug. 12. Its position at time of discovery was as follows: Aug. 12, 13 hours, 49 minutes, 27 seconds, Clinton mean time; right ascension, 21 hours, 20 minutes, 48.17 seconds; declination, south, 12 degrees, 29 minutes, 8.2 seconds. The daily motion of the object is —36 seconds in right ascension, and in declination 20 minutes and 50 seconds south. It is unusually bright for an asteroid, being of the ninth magnitude.

—The *Nation* for Aug. 2 calls attention to a very interesting feature of the table of ages (table XLII.) in the compendium of the tenth census. The table exhibits an astonishing preponderance of persons whose age is a 'round number,' i.e., a multiple of five or ten. One of the instances mentioned is, that while, according to the table, there are 1,094,324 persons at the age of 30, there are only 621,852 persons of 29 years, and only 492,530 persons of 31 years. There is a less powerful but still very marked and constant attraction to even numbers as compared with odd: for example, 42 claims 458,949, while 43 is content with 384,259; 47 is credited with 349,512, but 48 with 400,549. These are from the table of aggregates for the United States. The peculiarities are, of course, much more strongly marked in the columns referring to the classes and localities where there is most ignorance. Thus the number of the colored females in Mississippi who are put down as 30 years of age is 10,619, while the years immediately preceding and following are given only 2,253 and 1,236 respectively.

The writer of the interesting note in the *Nation* attributes the phenomenon to conjectural statements

by people who did not know their own ages; but probably only a small part of it is due to that cause, at least in the more intelligent portions of the population. In so intelligent a state as Rhode Island, for instance, we find for the years 29, 30, 31, the numbers 3,965, 6,550, 3,112; which is not much better than in the aggregate of the United States. How much is due to guessing by relatives, servants, masters, etc., and especially to suggestions and guesses by the census-gatherers themselves, — who, of course, do not regard the exact ages as important, and most of whom have probably no strong views on the subject of the 'personal equation,' — no one can tell, but probably very much more than to people's ignorance of their own ages. An examination and comparison of the original note-books of the various census-takers would furnish materials for an interesting exercise, if nothing more, in statistical research, and might reveal approximately the extent to which the personal qualities of the census-takers has affected the result; while a comparison of the table with well-established tables of mortality might enable us to estimate the force of the tendency to understate age which would doubtless be found to exist. The whole thing makes a very pretty problem, and serves to illustrate in a rather gross and exaggerated way the complexity of statistical investigations.

— We learn from *Nature* that a meeting which may have an important result upon science and art instruction in England has been inaugurated at Manchester. An association has been established to effect the general advancement of the profession of science and art teaching by securing improvements in the schemes of study, and the establishment of satisfactory relations between teachers and the Science and art department, the city and guilds of London institute, and other public authorities. It proposes also to collect such information as may be of service to teachers professionally; and it will endeavor, by constant watchfulness, to advance the status and material interests of science and art teachers in all directions. The president of the new association is Professor Huxley, and the vice-presidents are Dr. H. E. Roscoe, Mr. Norman Lockyer, Professor Boyd Dawkins, Professor Gamgee, Professor Ayrton, Professor Silvanus Thompson, Dr. John Watts, Mr. S. Leigh-Gregson, Mr. John Angell, Mr. W. Lockett Agnew, Mr. C. M. Foden, and Mr. J. H. Reynolds. Mr. W. E. Crowther, of the Technical school and mechanic's institution, Manchester, is the honorary secretary; and all communications should be addressed to him, especially by those who are desirous of forming affiliated unions in other districts. We believe that branches are already being established at Newcastle-upon-Tyne and Liverpool.

— The attorney-general of the United States has approved the title to the proposed site of the fish-commission establishments at Wood's Holl, Mass.; and the contracts for the work on the breakwater, pier, and basin, will, it is expected, soon be made.

— King's Dictionary of Boston, after the manner of Dickens's Dictionary of London, has recently been published. Edwin M. Bacon is the editor. A short introduction is written by George E. Ellis, D.D. The brief notices of the libraries and scientific associations of Boston are satisfactory, and well brought down to date.

— For the last two years a couple of buck mountain sheep have been running with the flock of Mr. Bailey of Bull Run Basin, Nevada; and there are now between twenty and thirty half-breed lambs in the lot. According to the Tuscarora mining news, they are mostly covered with hair, although there is some wool amongst it. They carry their heads high, like the wild sheep, but are as easily herded as those of pure domestic blood. They are of no value for shearing, but are said to make excellent mutton.

— The subsidence of land in the Cheshire salt-districts of England is again becoming alarming. The bed of the river Weaver has widened out below Northwich, forming a lake of about two miles square, called the Flashes. Crater-like holes suddenly fall in, forming in a day or two deep ponds of saltish water. In one instance, two years ago, the river itself flowed backwards into the subsidence for the space of two minutes, filling up several old rock-salt mines in the neighborhood: from these the water is now pumped, and used as brine. Land-owners in the neighborhood brought a bill into Parliament during the session of 1882, to obtain compensation for the damage done by the salt-works; but it was argued that subsidence would occur by natural filtration, even if the brine were unworked, and the bill was thrown out.

— Mr. Albert Marth, F.R.A.S., has succeeded Dr. W. Doberck as astronomer at Col. Cooper's observatory, Markree, Ireland.

RECENT BOOKS AND PAMPHLETS.

Albert-Levy. Les nouveautés de la science. Paris, *Hachette*, 1883. 192 p. 18°.

Alvarez, Llanos, C. Electricidad estática. Madrid, *libr. militar*, 1883. 238 p., illustr. 8°.

Bell, A. Melville. Visible-speech reader for the nursery and primary school. Cambridge, *King*, 1883. 4+52 p. 16°.

Bernimolin, H. Catalogues des plantes spontanées et cultivées du Tournaisis, avec indication des localités où on les rencontre. Tournai, *Vasseur-Delmée*, 1883. 133 p. 12°.

Beringer, A. Kritische vergleichung der elektrischen kraftübertragung mit den gebräuchlichsten mechanischen kraftübertragungssystemen. Berlin, 1883. 8°.

Brandza, D. Prodromul florei romane san enumeratiunea plantelor pana astade cunoscute in Moldova si Valachia. Bucuresci, 1883. 652 p. 8°.

Bureau, Th. Technologie des matières textiles. Gand, 1883. 235 p., 17 pl. et figures. *autogr.* 4°.

Carnoy, J. B. Biologie cellulaire; étude comparée de la cellule dans les deux règnes, au triple point de vue anatomique, chimique et physiologique. Lierre, 1883. illustr. 8°.

Centralbureau der europäischen gradmessung. Verhandlungen der vom 11 bis zum 15 September, 1882, im Haag vereinigten permanenten commission der europäischen gradmessung, redigirt von den schriftführen A. Hirsch, und Th. von Oppolzer, zugleich mit dem generalbericht für die jahre 1881 und 1882. Berlin, *Reimer*, 1883. 6+155 p., 2 maps. 4°.

Cervera Bachiller, J. Creencias y supersticiones, tradiciones, leyendas, consejas, historias místicas y preocupaciones populares de todos los siglos y de todos los pueblos. Madrid, *impr. Riva*, 1883. 304 p. 8°.

Chamberland, C. Le charbon et la vaccination charbonneuse d'après les travaux récents de M. Pasteur. Paris, 1883. 324 p. 8°.